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MEMOIRS

BY THE SAME AUTHOR.

THE PREVENTION OF MALARIA. With Contributions by 20 of the Leading Experts.

A SUMMARY OF FACTS REGARDING MALARIA. Suitable for Public Instruction.

PHILOSOPHIES.

THE SETTING SUN. A Satire in Verse.

PSYCHOLOGIES.

FABLES.

THE REVELS OF ORSERA. A Mediæval Romance.

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PLATE I.

GENERAL SIR CAMPBELL CLAYE GRANT ROSS, K.C.B.,
BENGAL STAFF CORPS.

[*Frontispiece*

M. B.
R.

MEMOIRS

WITH A FULL ACCOUNT OF
THE GREAT MALARIA PROBLEM
AND ITS SOLUTION

BY RONALD ROSS

23895.
22. 1. 29

LONDON
JOHN MURRAY, ALBEMARLE STREET, W.
1923

Inscribed

To the People of Sweden

and to the memory of

Alfred Nobel

RC
156
R84

Printed in Great Britain by
Hazell, Watson & Viney, Ld., London and Aylesbury.

PREFACE

THE solution of the malaria problem has been called the most dramatic episode in the history of medicine, and the facts when disclosed were certainly among the most wonderful in natural history; but the story has often been very inaccurately reported. Twenty years have elapsed since I first proposed to publish it in detail for medical readers—that is, with the correspondence and other matter now contained in Part II and in portions of Part III of this volume; but my numerous malaria-expeditions, and then the compilation of my textbook on the Prevention of Malaria, and lastly the war, interfered with the project.

The book is now complete; but I have decided to address it not only to medical men but to the general reader, in the form of my memoirs. There are several good reasons for this. In the first place it is hoped that the work will be of some practical use as regards the reduction of one of the most widespread of diseases. Now this is a matter which is always ultimately in the hands of laymen—it is they, not the doctors, who rule the world. Hitherto the matter has been left almost entirely to the medical profession, which, however, has failed to carry my scheme of 1899 to its logical conclusion, largely because it is allowed little influence in the world's counsels. Nothing, I am convinced, will really be done in this direction until those who govern us take the trouble to understand the subject. The only way to persuade them is to put the whole matter completely before them from my own point of view. In other words, the book is an appeal to Cæsar; and I hope that he will deign to consider it.

But it has another object. A witty friend of mine once remarked that the world thinks of the man of science as one who pulls out his watch and exclaims, "Ha! half an hour to spare before dinner: I will just step down to my laboratory

and make a discovery." Who but men of science themselves are to blame for such a misconception? Out of the many memoirs which fill our libraries few recount the labours of investigators, even of those who seek to solve the secrets of the great maladies which annually destroy millions of us—surely a matter of interest to everyone. Our books of science are records of results rather than of that sacred passion for discovery which leads to them. Yet many discoveries have really been the climax of an intense drama, full of hopes and despairs, visions seen in darkness, many failures, and a final triumph: in which the protagonists are man and nature, and the issue a decision for all the ages. I trust, then, that this book at least will give a frank and accurate picture of one investigation, of the difficulties which attended it, and of the manner in which the world received and used, or did not use, the result.

In writing it I have seldom trusted to memory alone, but have verified most of the details from the mass of documents —generally scribbled in the heat of action—which I possess. The book contains some criticisms which are required in the interests of human life or of science—it might usefully have contained more; but I have usually left the facts themselves to do the talking. Even for most of the misdoings mentioned here I must beg for the charity of humour because they were really due less to individuals than to that large and cheerful national indifference to all intellectual effort which our fathers called Philistinism. When this spirit applies merely to learning, art, and the more sidereal sciences it may perhaps be only lightly satirised; but what is to be said for it when it applies to matters of the life and health of whole nations? Allied to it is that administrative barbarism which allows, for instance, a regiment to become infected with malaria because the officials cannot agree as to who shall hang the bed-nets (page 501); or which permits death-dealing street-puddles to remain in the midst of crowded and suffering cities for years after the danger of them has been fully demonstrated to all (page 489).

There is nothing, I hope, in these pages which the lay reader cannot easily understand; if there be, let him pass it over and proceed! We have, I fear, too much of the "first person singular" (besides many other faults), but I cannot trouble to correct this now; and, lastly, some events which might have found place in the final chapter have been resigned to the eloquence of silence. In fact, the detailed history ends at 1902.

I pray that the book will prove to be something more than a medical tale. Perhaps the kindly reader will perceive that when my malaria-work was commenced thirty years ago as a self-imposed duty I had many ambitions closer to myself than it was. Indeed it was these ambitions which led to the malaria-work as an episode, not always agreeable, in my life. They had given me, together with other pleasures, the philosophy of Epicurus, Lucretius, Comte, and Spencer; and I make my boast—among many other bravadoes—that I am one of the few among these masters' disciples who have attempted to bring their teaching to fruition! To render this clear, I add the first Part of the volume—abstracted from a family record written during the war.

For the rest, the world is worth living in: my complaints are mostly for others, not myself; for science; and for human life: and I have few misfortunes to boast about. If happiness had no history this book would not have been indited; and I now have the pleasure of ending it exactly a quarter of a century after that memorable day which occasioned it.

RONALD ROSS.

41 BUCKINGHAM PALACE MANSIONS,
LONDON, S.W.1.
20 *August*, 1922.

CONTENTS

PART I.—INDIA

I. Sun, Mountains, and Wars. India, 1857–1865 3

II. Lessons. Hampshire, 1865–1874 . . . 18

III. The Comedy of Youth. Home, 1874–1881 . 29

IV. The Morning Stars. Madras, Bangalore, 1881–1885 42

V. Burma, The Andamans, Madras, 1885–1888 . 63

VI. Home, Burma, Bangalore, 1888–1893 . . 79

VII. Coonoor, Home, 1893–1895 100

PART II.—MALARIA

VIII. The Problem 115

IX. First Experiments. Secunderabad, 1895 . 132

X. False Scents. Secunderabad, 1895 . . 152

XI. The Scavengers. Bangalore, 1895–1897 . 179

XII. The Dappled-winged Mosquito. Sigur Ghat, 1897 199

XIII. The Discovery. Secunderabad, 1897 . . 214

XIV. Punishment. Kherwara, 1897–1898 . . 240

XV. The Proof. Calcutta, 1898 . . . 259

XVI. The Mode of Infection. Calcutta, 1898 . 291

XVII. The Third Interruption. Assam, 1898 . 314

XVIII. The Discovery discovered. Calcutta, 1898–1899 332

PART III.—THE FIGHT FOR LIFE

XIX. The New School. Liverpool, 1899 . . 363

XX. The Work completed. Sierra Leone, 1899–1900 374

XXI. Roman Brigandage. 1900 396

XXII. Mathematics, Malaria, Yellow Fever. 1900 414

XXIII. The Dash for Victory. West Africa, 1901 427

XXIV. West Africa, Ismailia, and Stockholm, 1902 455

XXV. Panama, Greece, Mauritius, Cyprus, the War, 1903–1922. 484

APPENDIXES

I. Honours and Awards 525

II. Honorary Membership of Societies . . 525

REFERENCES

I. Malaria and Mosquitoes 526

II. Mathematical Papers by R. Ross . . 534

III. Printed Literary Works by R. Ross . 535

IV. Miscellaneous 535

Index 537

LIST OF PLATES

I.	General Sir C. C. G. Ross, K.C.B., Bengal Staff Corps	*Frontispiece*
		BETWEEN PAGES
II.	Life-history of *Plasmodia* (with Description)	114–5
		FACING PAGE
III.	Charles Louis Alphonse Laveran. . . .	120
IV.	Patrick Manson	128
V.	Facsimile of Page in Laboratory Notebook, No. 1.	224
VI.	Ronald Ross and Laboratory at Calcutta . .	278
VII.	The Rt. Hon. Sir William MacGregor . . .	444
VIII.	Malcolm Watson	490
IX.	W. C. Gorgas, R. Ross, H. C. Weeks, on s.s. "Advance"	492
X.	Ronald Ross	500
XI.	Caricatures of R. Ross	518

PART I

INDIA

CHAPTER I

SUN, MOUNTAINS, AND WARS. INDIA, 1857–1865

I SHOULD belie my name were I not to boast a pedigree a yard long; but I regret that no monarch heads the list and that the last Earl of Ross who had the honour of belonging to the family was killed at Halidon Hill so long ago as 1333. In spite of the pedigree I infer that as everyone possesses two parents, four grandparents, eight great-grandparents, and so on, my ancestors at that time must have included not only all the nobility but also the whole population of Scotland and some foreigners in addition. From that earl we are said to be descended by junior branches from the Rosses of Balnagowan and Shandwick (Ross-shire)—one of whom, Walter, who died about 1520, was married six times, while another,

ERRATA

Page 22, line 2: for *Fausus* read *Faustus*.
Page 23, line 2: for " anothre " read " another ".
Page 56, line 35: for " at the end of " read " early in ".
Page 101, line 39: for " Maharaja " read " Nizam ".
Page 198, line 2: for " 1906 " read " 1896 ".
Page 200, line 39: after " from India " add " (except in Madras)".
Page 223, line 23: for " 1895 " read " 1897 ".
Page 234, line 23: after " larvæ " add " of *A. culicifacies* ".
Page 327, line 22: for " 1916 " read " 1896 ".
Page 348, line 1: for " say " read " Say,".
Page 353, line 13: for " aventurers " read " adventurers ".
Page 513, line 21: replace hyphen with dash.
Page 519, line 15: for " Elliott " read " Elliot ".
Page 529, line 32: for " G. Bastianelli " read " A. Dionisi ".
Page 531, line 15: for " Sociey " read " Society ".
Page 531, line 19: for " reply " read " replies ".
Page 534, line 19: for " ibid. Vol. XVI, p. 293 " read " Proc. Roy. Soc B., Vol. 94, 1923 ".
Page 535, line 34: for " Prayer " read " Petition ".

CHAPTER I

SUN, MOUNTAINS, AND WARS. INDIA, 1857–1865

I SHOULD belie my name were I not to boast a pedigree a yard long; but I regret that no monarch heads the list and that the last Earl of Ross who had the honour of belonging to the family was killed at Halidon Hill so long ago as 1333. In spite of the pedigree I infer that as everyone possesses two parents, four grandparents, eight great-grandparents, and so on, my ancestors at that time must have included not only all the nobility but also the whole population of Scotland and some foreigners in addition. From that earl we are said to be descended by junior branches from the Rosses of Balnagowan and Shandwick (Ross-shire)—one of whom, Walter, who died about 1520, was married six times, while another, Andrew, had ten sons and seven daughters and lost most of his properties except Mid Fearn (Ross-shire). His second son, Hugh, went to India, became a director of the East India Company, and owned properties called Kerse and Skeldon in Ayrshire. Andrew's great-grandson, Hugh, was my grandfather, who married Eliza, third daughter of Major William Watson, 35th Regiment, and Catherine Claye. Lieut.-Colonel Hugh Ross's portrait in oils (now in my possession) shows a quiet-faced, blue-eyed soldier of the Indian Army, in uniform, nursing his sword. His first cousin once removed, William, also went to India, rebought Shandwick (which had been sold), but was shot in a duel at Blackheath by another cousin, David Reid, in 1790. A lawsuit ensued many years later; and I find the following account of the duel, taken from *The Caledonian Mercury* "of the day," in the form of a newspaper cutting pasted into my father's illustrated diary of his and my mother's journey to India in 1856:

"DUEL AT BLACKHEATH BETWEEN MR. WILLIAM ROSS AND LIEUTENANT REID, BOTH NATIVES OF ROSS-SHIRE, IN SCOTLAND.

"Major-General Bruce, brother to Lord Elgin, was second

to Mr. Ross, and a Mr. Hamilton second to Mr. Reid. The following statement of this unfortunate duel, which happened on Tuesday morning at Blackheath, is drawn up by the gentlemen who were present on the field: The parties met at half-past six o'clock morning, at Blackheath, when every endeavour in the power of the seconds was made use of to make up the unhappy quarrel between them; but finding it impossible, they agreed to fire at twelve paces distant. Mr. Reid received Mr. Ross's fire first; he then fired in return, and likewise without effect.

"The seconds again remonstrated with the gentlemen, and desired they should go no further, both having acted as became men of honour. Mr. Ross then insisted on an apology from Mr. Reid for the insults offered to him by Lieutenant Reid and his brother Andrew Reid, without which he could not appear amongst his friends in the *world*. Upon this Mr. Reid positively declined making any apology whatever. The seconds then declared to both that they could not in justice to their own characters stand by and see them put an end to each other. But Mr. Ross insisted positively on firing once more, which they did at the same distance as before, Mr. Ross, however, receiving Mr. Reid's fire first. The ball took place in his head, and he fell immediately without returning it. He was taken to his house in Wimpole Street and died in an hour after. In justice to Mr. Ross the seconds declared that before the first fire he solemnly asserted his innocence regarding the original cause of the dispute.

"London, May 17, 1790. The coroner's inquest that sat upon Mr. Ross, lately killed in a duel at Blackheath, have brought in their verdict—*Wilful murder*."

I do not know if the Ross of whom Butler wrote in *Hudibras*—

> There was an ancient sage philosopher,
> That had read Alexander Ross over,

was a relative, but my uncle of almost the same name told me he thought so; and my father said the same of Dame Margaret Ross, the mother of the Bride of Lammermoor—a terrible lady.

Lieut.-Colonel Hugh Ross (who died comparatively young at Cawnpore in 1838) had six children: Hugh, a brilliant student at Edinburgh, who also died young; Campbell Claye Grant

Ross, my father; Lieut.-Colonel William Alexander Ross, F.G.S., of the Bengal Artillery; Charles Edward Ross, a doctor; Eliza Jane, afterwards wife of Captain Barwell; and Adelaide Anne, wife of General John Tytler, V.C. Colonel Hugh Ross had a brother, Alexander, who seems to have enjoyed independent means and to have lived at 14 Buccleuch Place, Edinburgh. He was known in the family as Uncle Alec—a bachelor who resided also at Dresden, admired the Germans somewhat as Carlyle did, and was thought to be a misogynist, a cynic, and even a philosopher! I regret that I never saw him and have no papers regarding him.

My mother was English—Miss Matilda Charlotte Elderton. My cousin Ernest Christopher Elderton tells me there is a tradition that the family is descended from the Druids; and certainly our ancestor James Elderton lived at Orcheston St. Mary, near Stonehenge. His son Harbin Elderton bought the freedom of the City of London in 1756; and must have cultivated music, because he is described as Musician on the certificate, though he was a rich Russia Merchant. His elder son James was Deputy Remembrancer of the Court of Rolls; and his second son, Merrick, was Master's Clerk in Chancery, with a salary of £2,000 a year, and owned Effra House at Brixton, with three acres of garden, ten acres of fields, and a boat-house on the river called Effra. He married Hester Pierce, had four sons and three daughters, and died in 1836. His second son, Edward Merrick Elderton (my grandfather), was a lawyer in London, and owned Effra House, where my mother was brought up; but he sold it later—the estate has now become a crowded area—and moved to 73 Warwick Square. He married three times: first Miss Matilda Halford, daughter of Joseph Halford, Esq., of Charlemont Hall, Staffordshire. By her he had one son, Captain Edward Halford Pierce Elderton of the 26th Regiment (Cameronians), and one daughter, my mother—shortly after whose birth she died. Edward Merrick then married a Miss Carnegie, by whom he had three children, and, lastly, Miss Charlotte May Marcella Cameron, granddaughter of Lieut.-General Sir Alan Cameron, K.C.B., of Erracht, who raised the 79th Cameron Highlanders in 1793. Edward Merrick died in 1875, aged 72. He stood as parliamentary candidate for Merthyr Tydfil in 1867, but withdrew in 1868.

In the album of my grandmother Matilda Halford there are some exquisite pencil drawings by her, including one of her father's house, and another of Hallaton Hall, Leicestershire,

belonging to one Richard Halford, Esq., and there is an engraving of Wistow Hall, Leicestershire, then the seat of Sir Henry Halford, Bt. My mother always told me that we were related to the last—but I have searched in vain for details of the relationship. He was originally a Dr. Vaughan, who practised in London, became physician to George IV, William IV, and Queen Victoria, was President of the Royal College of Physicians for many years, and, when the old Halford Baronetcy at Wistow became extinct, was given the title and was (?) bequeathed the estate because of relationship with the Halford family. A book about him and several biographies are extant; but, like many other eminent doctors, he does not seem to have been a man of any fame, or even name, as regards medical science. Wistow is now owned by Lord Cottesloe, who has kindly tried, without result, to ascertain further facts for me on this point.

My grandfather Ross's children were all blond and good-looking, rather above the average stature, with bright blue eyes, brown hair, and straight noses. Although they were educated in Edinburgh (the sons at the Academy, I think), only my aunts spoke with any trace of a Scots accent. My grandfather Elderton's children had brown eyes and hair; and of my parents' ten children, four were blond and six had brown eyes. I belong to the latter group; but friends say that my eyes are sometimes grey, and enemies, that they are sometimes almost green! Both sides of the family were very fond of painting and music. I have numerous paintings by my father—three portraits of myself when a child by my aunt Eliza Barwell, and good water-colour figure-drawings in sepia by my uncle, Charles Ross. My grandmother Halford and my mother painted flowers beautifully. My father had a fine baritone voice and sang duets admirably with my mother—those dear old songs, "Flow on, thou Shining River," and "Sing, Sweet Bird," and many others, now displaced by a noisier generation; and he even composed several airs to which I set piano accompaniments in 1881. He was full of quotations from Shakespeare, Milton, Pope, and Byron, but hated Wordsworth.

Perhaps the soundest of all religions is hero-worship; and, next to that, ancestor-worship is a virtue if not a religion because it tends to keep us up to the mark. I have always had a great admiration for my uncle William Alexander, and only regret that owing to many preoccupations and absences from England I do not know more about him. He was a

brilliant, witty, and critical man, but without sarcasm; and my father said that he injured himself greatly in the Service by these qualities. I know that when I was a boy he invented a wonderful gun-carriage, of which he had a complete model made, with horses and riders. So far as I remember it was based upon the principle that a shot striking it would rotate the circular ammunition box and would therefore glance off without exploding the contents—a somewhat doubtful proposition. Anyway the invention was rejected by the War Office, much to my uncle's indignation. He married Henrietta, second daughter of the distinguished Indian officer General Sir William Sleeman, K.C.B., who suppressed Thuggee in India. She was born at Jubbulpur on 10 February 1838,[1] a very charming lady; and had two sons and five daughters (I think)—the former were killed later in South Africa. The reason why I admire my uncle so much is that he was that rare kind of persón, a true scientific investigator, who simply lost himself in his researches. He was always passionately addicted to the vice of blowpipe chemistry, and actually gave up the Service and took the small pension attached to the rank of major in order to prosecute these investigations, which he thought would bring him great honour. I visited him frequently in his house in Shepherd's Bush, in London, when I was a medical student about 1875–7. Needless to say he was far from wealthy; but he had a laboratory in the basement of his house where he used to toil all day at the blowpipe. He was a tall, fair man with copious Dundreary whiskers, and generally wore in his laboratory a long flowered dressing-gown, so that he looked like a mediæval alchemist in the midst of innumerable chemical apparatus. I used to have supper with him on Sundays, when he would deliver himself of many witty or wise remarks—not always combined together; and on delivering an epigram, he would pull his long nose with a sweep of his right hand to give emphasis to the point of the jest. In blowpipe chemistry one makes a bead of boric acid on a platinum loop; this is then fused under the blowpipe and a granule of the mineral to be analysed is added. I suggested to him that we might examine the resulting bead under the microscope, and remember that I actually made a study of a number of such beads and found some remarkable effects; but he did not look with favour on my suggestion. About the same time he brought out a fine book on *Pyrology*, which

[1] I am indebted to Colonel James L. Sleeman, C.B.E., M.V.O., F.Z.S., for being so kind as to ascertain these details for me.

he said nearly ruined him; and later, another book called *A Manual of Blowpipe Analysis*, which I suppose ruined him entirely. The copy of the first which he gave me was stolen from me in Madras about 1887, and the book appears now to be unobtainable; but his subsequent book called *The Blowpipe in Chemistry, Mineralogy and Geology*, by Lieut.-Colonel W. A. Ross, R.A. (Retired), F.G.S., Member of the German Chemical Society, with 120 illustrations by the author, is still in use. A reprint of the second (1889) edition was issued in 1912 by Crosby, Lockwood & Son, 7 Stationers' Hall, Ludgate Hill. My uncle travelled and studied much in Germany, and this book is dedicated in German to Professor Bruno Kerr, Königl. Bergakademie in Berlin. In his Preface he exclaims: "As regards my own experience, I can only say that this study has proved the solace and hope of a very chequered life, and that the retrospect of my humble labours in this new field is a constant and unfailing source of pleasure to me." He also maintains that the student shall use blowpipe chemistry "as a key of fire to unlock the secret and solid stores of nature; as an instrument of torture to force her to confess how she colours her amethysts, emeralds, and sapphires; as a 'pencil of light' wherewith to trace, in imperishable records and with electric rapidity, the precise composition alike of her soft sulphurous ores, or her adamantine corundum." Evidently an enthusiast, poor man; for he often told me that he could not get a single chemist in Great Britain to look at his work and his discoveries. He died, I believe, much disheartened, about 1889. But when I went to the great Congress of Arts and Sciences held in connection with the International Exhibition at St. Louis in the United States in 1904, on my way to Panama, I was introduced to a distinguished American chemist as Major Ross. He shook me warmly by the hand, and said, "It is an honour to meet you, Major Ross; I have long admired your great books on Blowpipe Chemistry"! He told me that my uncle had made many advances in this subject, especially in his invention of the Aluminium-Plate Support. As a matter of fact, nearly all the ideas in science are provided by amateurs, such as my uncle Ross; the other gentlemen write the text-books and obtain the professorships.

A relative whom we all honoured very highly was Brigadier-General John Adam Tytler, V.C., C.B., who married my aunt, Adelaide Ross. He was the son of Dr. John Tytler, of the Indian Medical Service, and a relative of Lord Gillies, and joined the Indian Army in 1844, being commissioned in the same

regiment as my father, the 66th Bengal Native Infantry. He served in many frontier campaigns; and in the Indian Mutiny distinguished himself in the action of Churpura, fought on 10 February 1858, by riding up alone to the enemy's guns under a heavy fire, until his men could come up. There he remained engaged in a hand-to-hand encounter, was shot through the left arm, received a spear wound in the chest, and had a ball through the right sleeve of his coat. The guns were then taken by our troops, and he received the Victoria Cross for his gallantry. Later on he raised the 4th Regiment of Gurkhas, which he commanded for a long time. On the outbreak of the Cavagnari War with Afghanistan, he was appointed Brigadier-General, but ill-health compelled him to resign and he died of dysentery shortly afterwards. A tall, spare man, with a complexion bronzed or rather yellowed by Indian service, with extremely firm lips and a spare moustache, he was notorious for his complete calmness in all circumstances. My father used to say that he was known only once to show any signs of anger, and that was when he had taken an Indian village the people of which had previously massacred a number of English ladies and children. The village butcher had been employed by the villagers to commit the crime, and when the village was taken this man cringed before my uncle like Agag before Samuel, and then it was that Tytler really lost his temper. An obituary notice of him appeared in *The Illustrated London News* for 6 March 1860. He gave me the only copy of Shakespeare which I possessed for many years.

My father, Campbell Claye Grant Ross, was born in 1824; his commission in the Indian Army was dated 4 April 1841; and he arrived in India on 2 July as Ensign and was attached to the 66th Bengal Native Infantry, with which he served in Arrakan and at Dinapore and Lucknow until 1850, without seeing much active service. But, like most of our officers, he was very fond of shooting and fishing, and left two delightful sketch-books describing many of his sporting expeditions and the Indian scenery, with numerous water-colour sketches. He wrote in a round, bold handwriting, with scarcely any erasures; and I think that he possessed almost a unique style in water-colour—broad washes of rich colour, full of light and shade, often fined and pointed with body-colour or Indian ink, and generally done so rapidly that the picture was finished in an hour or two, sky, mountain, forest, and water, the snow of the Himalayas gleaming above the deep-green massed shades of the jungles—all in a few touches, which, when I was a

boy, I tried in vain to emulate. Throughout his life it was his greatest pleasure to take his brush and draw "the things that he had seen," and also to perfect the presentment of them; for, swiftly as his brush moved, his mind yet anxiously balanced every part of the drawing as it grew under his hand. His pictures have mingled in me with my own memories and have become a part of myself; yet I believe that he scarcely ever tried to exhibit them, and they were done only for his own pleasure and for that of his family. But he was not good at human figures—or rather did not trouble to attempt them. "I love not man the less," he quoted, "but nature more."

But in 1850 things changed. His regiment had been ordered up to Amritsar to relieve the regiments which had fought the Sikh campaigns, but it was not given the higher rate of pay which the latter battalions had enjoyed. In consequence the whole regiment mutinied in a passive manner, and Sir Charles Napier, the Commander-in-Chief, disbanded the disgruntled men and substituted the Mussuri Battalion of Gurkhas (March 1850). This gave the officers a year of service at Simla, and then at Peshawur and Rawal Pindi, where the 66th Gurkhas were engaged in various frontier wars—the assault of Pranghur, the fight at Iskakote, and the Bori campaign of 1853. In April 1854 my father visited Kashmir with two friends and had some good fishing—chiefly for what he calls Kashmir trout, with fly or minnow—some bear and deer shooting. He then went home on furlough on 6 March 1855, married my mother at St. George's Church, Hanover Square, London, on 7 July 1856, and returned to India on 4 January 1857, when he was sent with his regiment to Almora in the Kumaön Hills—situated about the centre of the great Himalaya Range, north-west of Nepal. I was born there on 13 May 1857—three days after the outbreak of the great Indian Mutiny.

My father's illustrated diary of their voyage out to India round the Cape of Good Hope in the sailing ship *Marlborough* is a most interesting family possession. They left London (18 Upper Eccleston Place) at 6 a.m. on Thursday 11 September 1856, reached Portsmouth at 11.30 a.m., and set sail at 6 p.m. They sighted Madeira on 21 September, and crossed the Line on 12 October (Neptune of course), passed Tristan da Cunha on the 30th; had a bad gale on 18 November, but then travelled 213 miles a day, and reached the Ganges on 2 January. In Calcutta they went into a boarding-house, where four of the other passengers who had gone there with them all died of cholera within three days of arrival (page 64). They reached

Almora on 6 February, and heard of the Mutiny on 15 May. My aunts Eliza and Adelaide were with them all the time and were married later in India.

As the Gurkhas are a hill tribe, the 66th Regiment was kept in garrison during the Mutiny in the Kumaön Hills, either at Almora or the neighbouring hill-station, Naini Tal. Associated with them there was a company of Pandi artillerymen, who were likely to mutiny—while the Gurkhas, of course, remained loyal throughout. One morning two of the Gurkhas informed Captain Ross (who was second in command) that they had overheard the artillerymen plotting to massacre the British officers and their families. My father and my uncle Tytler (who was Adjutant) reported the matter to the Colonel (McCausland); but that easy-going officer refused to credit the information. A day or two later, however, other Gurkhas made a similar report; and now my father and uncle persuaded the colonel to take action. A parade was ordered at which the unarmed Pandis suddenly found themselves surrounded by the armed and loyal Gurkhas and were obliged to submit—but for which event this history would never have been written. With the usual absurd clemency of our politicals, however, the whole company of artillerymen was ordered, later on, to be released, and, of course, joined the mutineers in the plains.

The district of Rohilcund, peopled by the warlike Mohammedan race of the Rohillas, lies in the plain at the foot of the Kumaön Hills, and was soon in open rebellion. From July 1857 my father was stationed at Naini Tal, which is nearer the plains than Almora, in order to watch attacks threatened from below; and I have his letters to my mother, who remained at Almora. In February 1858 two armies of Rohillas, numbering about 3,000 men, with cavalry and guns, moved to the foot of the hills with the boasted intention of taking Almora and Naini Tal and destroying us all; but instead of ascending the hills they spent night after night in revelry and natches on the plain, and the light of their camps could be seen from the mountains above. Colonel McCausland had a force of about 500 Gurkhas of the 66th under Captain Ross's command, and 400 men lent by the Jung Bahadur, with some cavalry and guns, and, according to my father, laid his plans admirably. On Tuesday, 9 February 1858, they left camp secretly, descended the hills, and, marching all night, arrived close to the enemy towards morning. They were given an hour's rest and then rushed the mutineers at dawn, carrying the camp, taking guns, treasure, standards, and prisoners, and

destroying the whole army except a few who escaped on camels. This action was known as that of Churpura. At one spot the mutineers attempted a stand; when Tytler took their guns and was dangerously wounded as described, while all the officers experienced hand-to-hand fighting, and my father broke his Wilkinson sword in a big mutineer, whose sword he took leave to keep in exchange. The Colonel gained his Companionship of the Bath, my father was made a Brevet Major, and Tytler was given the Victoria Cross. The other body of mutineers melted away when it heard the news, and in May Sir Colin Campbell recaptured Bareilly. The action had saved the hill-stations, where so many of our women, children, and sick had taken refuge. We suffered about forty casualties; but the enemy lost heavily from the Gurkhas' kukris (short, broad, curved scimitars kept as sharp as razors); and my father used always to tell that he saw the body of a natch girl cut absolutely in half at the waist by a single stroke of a kukri in the night charge.

After the Mutiny, in 1861, my father, though only a Major, was given command of that fine new regiment the Ferozepur Sikhs (afterwards the 14th Sikhs); and in 1863 his corps was employed in the Ambeyla Campaign, in the mountains near Peshawur, on the North-West Frontier. He described the details of the campaign in a most interesting series of almost daily letters to my mother. The force collected at Nowa Killa, near Peshawur, in October 1863, and by 1 November began to occupy the Ambeyla (or Umbeyla) Pass. The tribesmen were slow in collecting, but attacked first on the 30th, and then very seriously on Friday, 13 November. My father's account of the affair is the following:

"I, as senior officer, have the command of the advanced pickets, Key's, Brownlow's, and my own corps. One of these pickets is on a high crag overlooking camp. I put Brownlow into it with the whole of his regiment, and 30 of mine, and the 25 [? 15] European marksmen. They were attacked incessantly all night long by the enemy, but beat them off. This morning the enemy attacked the whole of our pickets in great numbers, especially two very low down the hills and detached. As the firing got smart down there I considered it my duty to go down there and found the picket hotly engaged. When the firing slackened a little I returned, and on reaching the foot of camp you may imagine my horror to see the top

picket carried by hundreds of the enemy, who with loud yells turned a fearful fire on us below. Everything was in the most terrible confusion at once, the mules, etc., and the camp-followers rushing to the rear and blocking up the roads. We got as many of the men who came with us as we could and with some men of the other corps made a desperate attack on the crag, but the fire was so hot and they rolled rocks and stones on us so hard that so few of us could not manage it and we got among the rocks and held our place close to the enemy (10 paces) and kept firing them down. Just then the 101st came up and with them we set up a loud hurrah and took the crag from the enemy. When we got to the top we had a heavy fire sent into us, and fancy what a near shave I had of it. I got my trousers cut by *three bullets* and they never touched me. A fourth just touched me on the thigh, stinging me like a burn and went right into a European soldier. I am afraid he was killed, poor fellow, but I could not stay to see. . . . We had 18 men killed and 16 wounded. This morning there is not an enemy in sight. Yesterday was Friday, and they will not do anything more till next Friday."

Nevertheless on the 18th, they had another stiff fight when on picket duty, 1 officer and 26 men being killed and 45 wounded out of 130 men engaged. "A clumsy *fool* of a European soldier stumbled and ran his bayonet into my face and has given me a painful wound. . . . The whole brunt of the day fell on our corps and capitally they stood it." The Commander-in-Chief telegraphed to the General (afterwards Sir Neville Chamberlain) telling him to thank Ross and Brownlow, and the regiment was so cut up that it was given a rest. A British soldier was heard to say, in the usual manner of Mr. Thomas Atkins, "That chap Ross deserves a better command than a black regiment," and my father adds, "I am prouder of this last compliment than the Commander-in-Chief's, because the European soldiers have no humbug in them." A fifth bullet went through his puggri and turned his cap on his head. The General himself was wounded later; but reinforcements soon arrived (John Tytler and the 5th Gurkhas with them, and also Major Roberts, ? afterwards Lord Roberts), giving a total of about 9,000 men; and on 15 December the General made a grand attack on the enemy and entirely routed them, thus practically ending the campaign.

The weather was bitterly cold and wet most of the time; and my father had three returns of his old "Peshawur fever" (malaria), namely on 7 October, 25 November, and 15 December, at 2 p.m., in the middle of the battle—yet he had no thoughts of "going sick" of course, but was ultimately put "through a regular course of quinine." All his letters contain messages to us children, then three in number; and I had written him a child's letter in which I seem to have suggested—much to his amusement—that guns should be protected with spears! They were home again shortly after Christmas; and Major Ross was made a Brevet Lieut.-Colonel.

After this the 14th Sikhs were stationed for a time at Mian Mir, close to Lahore, the station where, many years later, the medical authorities endeavoured to discredit, by means of a meretricious experiment, the possibility of reducing mosquitoes according to my suggestions (page 505). In 1865 we were marched all the way down to Benares, from which I was sent home to England. My father remained in command of the regiment until 1875, except for two intervals in 1868 and 1873, when he was home on furlough; and after that was appointed Brigadier-General in command of the very important military district of Peshawur. In 1877 he led the punitive column from Peshawur against the troublesome Jowakis, a tribe of Afridis, while Brigadier-General Keyes led another column against them from Kohat; and on 6, 7, and 8 December 1877 they destroyed the seven robber villages of Bori and surveyed and mapped the country for the first time. In 1878, Lord Lytton being Viceroy, trouble began with Afghanistan, and, as my father held the principal frontier command, his position was a very responsible one. He always told me that in the autumn of that year he was urged privately by the authorities to seize Ali Musjid, the Afghan fort in the Kyber Pass, on his own initiative, so that we could command the pass if war were to break out; and I have letters dated 3 and 5 October 1878 from him to my mother showing that some action like this was contemplated. But though the force under him was sufficient to guard the plain of Peshawur, it was not sufficient to enable him to attack the fort with a certainty of success, especially as a large part of his army was down with the usual malaria of that season; and the suggestion was therefore abandoned. The result was, however, that when war did break out upon the murder of Sir Louis Cavagnari at Kabul on 3 September 1879, my father was not given the important command of the troops

which marched to Kandahar under General (afterwards Lord) Roberts, but was kept at the base in Peshawur. He was, however, made Knight Commander of the Bath and was given the presidency command at Calcutta, temporarily, previous to his being finally placed on the unemployed list after thirty-nine years' service, with the rank of Major-General in 1880, Lieutenant-General in 1886, and full General in 1890. He lived for twelve years at Lothian House, Ryde, Isle of Wight, and died of diabetes at Eastbourne on 20 June 1892, aged 68. A short biography of him appeared in *The Cosmopolitan* for March 1888 (London, Digby & Long).

The curious Ali Musjid affair was well analysed by Mr. Archibald Forbes, the well-known war correspondent, in *The Civil and Military Gazette* for 7 May 1879. After criticising the action of Lord Salisbury and of Lords Northbrook and Lytton with regard to the Amir of Afghanistan, he says :

" Immediately after the repulse of Sir Neville Chamberlain's mission in the beginning of September last (1878), the Viceroy issued orders through the regular channel, the Commander-in-Chief, to Brigadier-General Ross, commanding at Peshawur, to go and drive the Ameer's garrison out of Ali Musjid and hold the place. . . . Brigadier-General Ross is a soldier who has shown his capacity again and again. . . . He got his orders, and he promptly mustered his available strength. He found that when he left behind only three hundred men, chiefly convalescents, to overawe the most turbulent city of Upper India, in which disaffection was known to be rife, there was forthcoming for the prescribed enterprise a force barely one thousand men, in whose ranks were many men whose efficiency fever had deteriorated. Not less morally than physically brave, General Ross rightly thought it his duty to represent the great risk of disaster which offensive operations of an indefinite character with this handful of virtually unequipped soldiers would entail. His arguments were too cogent to be disregarded, and the crazy scheme was abandoned."

Nevertheless the authorities ordered the chief commissariat officer at Peshawur to have seven days' supplies ready for six thousand men for the advance ; and the rash design seems to have been checked only by orders from home. Mr. Forbes adds : " These fortunately abortive struggles to compass premature hostilities are now for the first time made public."

So far as I can gather, Lord Lytton had wished to make a dash upon Afghanistan before his army was ready for the task. Of course, Mr. Forbes did not derive any of his information from my father.

General Sir Campbell Ross was a typical soldier, straight, stern, downright, and greatly experienced in the fierce campaigns against the warlike hill-tribes of India. I have letters to him from a number of distinguished officers, including Cavagnari, Colley, and Lord Roberts. He liked Colley, but thought that he was an academical soldier; and I remember him crying out, "I knew it," when the news of Majuba Hill was reported in the home papers in February 1881. Of the arts of civil commissioners and politicians he was, not scornful, but very sceptical, and he thought it a mistake to dilute the authority of the general in the field by the presence of political officers. Of rights, grievances, and politics he was oblivious; he had no delusions about the equality and liberty of men; and his own watchword was Service—a far nobler thing. But his spirit was also loaded with the gorgeous colours of the world's beauty, and his brush was his daily companion to the last. Such were the men who made our empire—and, more than that, gave peace and prosperity to a continent. I wrote of him later in my *In Exile* [112], but the stanzas were omitted from the published copy [114].

Like all mothers, our mother was the best in the world. She bore ten children; and only those who remember India at that time can know what this means. The heat, the flies, the mosquitoes, the poor food for infants, the ever-imminent risk of illness, the annual journeys in palanquins or bullock-carts over the insufferable plains to and from the hill-stations, the long partings from husband or children, and the anxieties of war-time made martyrs of our mothers in those days, and often do so still: but of mine I do not write here. She painted flowers exquisitely and sketched well, but without my father's dash; she had a true and sympathetic voice; and she taught us not of earth but of heaven, and showed us the way there. She died at Putney on 31 May 1906, and is buried with my father in Ocklynge Cemetery, Eastbourne. Their children were myself, Marion Adelaide, Alexander, Claye Ross, Charles, John, May, Isabel, Hugh Campbell, and Edward Halford, in order of birth.

The British in India in those days afforded a spectacle which is, I suppose, unique in history, except perhaps in the case of the early Spanish conquerors in South America. Mostly

scions of the younger branches of old British families, they themselves constituted in India a true aristocracy which has probably never been excelled in the beneficence of its achievements. Superior to the subject peoples in natural ability, integrity, and science, they had found a vast but decadent population broken up into innumerable petty states at constant war with each other, sunk in superstitions, deficient in courage and rectitude, plundered by priests, money-lenders, and inefficient officials, housed in hovels, and swept by annually recurring epidemics. They introduced honesty, law, justice, order, roads, posts, railways, irrigation, hospitals, defence from external enemies, and, what is essential for civilisation, a final superior authority. But they themselves dwelt apart like the gods: they could not endure those vast sun-swept plains; they were obliged to fly frequently to the cooler mountains, to send their children home to the little, distant, misty island of the far Atlantic, to return there themselves at last to die. Rich men in India, they were forced to fall into comparative poverty at home; and men who had governed great provinces or commanded large armies became less in England than wealthy tradesmen and their children, who were now beginning to compose the new aristocracy of Britain. I think that they constituted a caste apart from that of the stay-at-homes. They were always doubly in exile. Among the fires of India they yearned for the cool pearly skies and the flowered woodlands of the homeland; but then, when they had returned, they still dreamed of the mighty suns, mountains, and plains of India. So too their children born in India. For myself I prefer such a heritage to any political or commercial one: but it has its disadvantages: we suffer in health, and our memories are apt to make us, I think, almost aliens even in the land of our fathers.

CHAPTER II

LESSONS. HAMPSHIRE, 1865–1874

As already stated, I was born at Almora in the Kumaön Hills, about the middle section of the great Himálayan Range, on Friday, 13 May 1857 [1]—three days after the Indian Mutiny broke out. My first memories are of mountains, snows, rhododendrons, and fir-trees ; but the years of childhood which *precede* the birth of memory are always of great psychological interest. The small child is able to reason ; he knows what he wants and how to get it ; he has his emotions and passions and all the elements of what we call mind ; and yet, later, he can remember nothing of what has happened to him before the age of about five years. Is it possible that his brain-cells are changed at that age ? Does he shed his first mind, so to speak, a little before he loses his first teeth, as a caterpillar sheds its skin—an intellectual ecdysis ? The philosophers of the Berkeley School, who argue that mind is given to us at birth by some supernal inspiration, are strangely silent about this period. Yet during it we learn speech, many co-ordinated movements, judgement, self-control, fear, affection—probably all by experience only. Then suddenly the memory of these experiences vanishes, while the technical education which they have given us remains ; and we start afresh with a new mind for our life-work.

Like other children I was taken to the hills (Naini Tal or Dalhousie) before every hot weather ; and, to judge from three water-colour portraits of my little self by my aunt Eliza, I must have been a remarkably healthy child up to 1860. But my mother told me that I had once nearly died of dysentery ; and a photograph of me seated on my father's knee and dated July 1860 shows a very sickly child indeed. I remember nothing of the illness.

[1] I am told that 13 May (old May Day) is the most unlucky of all birthdays. Those who enter the world on that day may, however, console themselves by attributing all their failures to misfortune and, therefore, all their successes to merit !

Our first recollections resemble sketches thrown unassorted into a scrapbook, without date or locality. Perhaps my first one was that of a great, dry, hot plain, with the setting sun and clumps of large trees. Two dhooleys (palanquins) were on the ground, the dhooley-bearers standing ready beside them. In one my father was groaning with fever, and my mother, who was seated with me close by, told me that she feared he was going to die—then the mist of forgetfulness closes. I remember punkas, kuskus-tatti-fans, dead game, deer, and a leopard lying in the veranda ; and the morning sun blazing on the edge of the plain. There is no memory at first of heat or cold, hunger or thirst, stars or flowers, horses or dogs or other live animals, or of music, conversations, or thoughts. I clearly remember making one discovery—that when my sister and I left the company of the Indian servants and were brought into that of our parents something extraordinary happened to our speech—we spoke English instead of Hindustani, equally naturally : when I returned to India in 1881 I had forgotten every word of the latter. I remember the smell of wetted dust, kuskus grass, and sandal-wood powder, and the taste of dhal, mutton (? goat), mahseer fish, and the Indian sweets which the servants gave us.

All this probably when I was about five years old. Then I remember going to " the hills." Where it was I do not know, but I once saw a gigantic snowy mountain by daybreak with one luminous star just above it. On another occasion, in a wood of lofty trees upon a hillside, I saw a mass of snow on the ground with flakes of fallen rhododendron-blossom upon it. One day I went with a large picnic party along a level road running round a hill (at Naini Tal), with a panorama of snowy mountains. After that I remember the long march made by us with my father's regiment, the 14th Sikhs, from Peshawur in Northern India to Benares in 1864. We were awakened before daybreak every day, given some delicious hot coffee, and put into a bullock-cart (European), with my mother, sister, and baby brother Claye—of course my father rode with his regiment. At about 9 a.m. every morning we arrived at the new camp ready pitched for us (two complete sets of tents were used on these marches, so that one set always went in advance). I dimly remember the towers of Agra and the Taj Mahal, and then, after some months' march, the minarets of Benares, and the Ganges with its wide-winged boats.

By this time I was seven years old. I remember nothing more of Benares, except seeing a railway-train for the first

time, and being fired with curiosity as to how it could move by itself. My father told me of Watts and the kettle, and I drew a diagram of what I supposed the machinery was. I thought the steam was emitted from the boiler in a jet and blew a wheel round! My mother showed me this diagram some years ago—evidently an anticipation of turbine engines! Curiously enough, I do not recollect a single *person* in India except our own family. I remember to-day the terror of a dream of mine in which my mother receded from me, though I followed as fast as I could—and then suddenly became a star and floated away to a vast distance. That dream was a true thing which happens to all of us; but the star never disappears entirely.

In 1865 it was time for me to be sent to England for health and education, and I was accordingly put in charge of my Aunt Eliza, whose husband, Captain Barwell, was in command of a party of soldiers who were being sent home by a Green Line clipper sailing ship called the *Lady Melville*, round by the Cape—a four or five months' voyage. Leaving Benares for Calcutta by train, we embarked on 15 April, I believe, and reached England in August or September 1865. I do not remember parting from my family nor the embarkation, but recollect the muddy water of the Hugli becoming green as we neared the sea. My uncle Barwell was a disciplinarian, and cured me of several nervous tricks, due to illness, by punishing me when I gave way to them—the proper way to treat such and other weaknesses. There was on board a very good young man (I heard he died a little later) who continued teaching me (what my mother had commenced) to read and write, and told me innumerable stories out of history and the Bible and even of philosophers and poets—of Alexander, Cæsar, Napoleon, Homer, Dante, and Socrates. I also remember Neptune coming aboard at the Line, several deaths at sea, whales spouting on the horizon, a great shark dragged in amidships, and the sport indulged in by some of the passengers of catching sea-birds by hanging out lines astern. One day they caught an albatross, which vomited on the deck. There were flying-fish, jelly-fish, porpoises, and nautiluses, of course. A child fell overboard one day, and on another occasion someone threw himself overboard: both were rescued. An old sailor made me a full-rigged ship and taught me the name of every spar, sail, and line—I have forgotten them all now. This sailor was tattooed all over and told me many tales; in return for which I put him into my novel *The Child of Ocean*. My

eighth birthday was passed on board. But my most vivid memory is that of the gloomy, mountainous St. Helena, and how we climbed the hill to see the tomb of Napoleon—a sandy oblong patch, surrounded by railings and empty (since the remains had been removed to France). I remember also the beautiful summer evening when we first sighted the Needles; but I have no memory of landing, nor of being taken to my uncle, Dr. William Byam Wilmot, in the Isle of Wight, with whom I was to live for some years.

Dr. Wilmot had practised in Australia, where he had married and raised a family, now grown up. His wife died; he retired on "very moderate" means, and lived at 2 Lyndhurst Terrace, Ryde, after marrying my mother's aunt, Harriet Elderton. Her sister Emma lived with them there. They were then all old people. Dr. Wilmot was a "dear old man," who I believe spent his time writing theological works, and who possessed a small general library. For my aunt Harriet I had the greatest affection, for she became my second mother. Calm, wise, and even learned, she set me the best example of life; and she painted beautiful delicate landscapes, chiefly of Westmorland. I mentioned her in my *In Exile*, though the stanzas were omitted from the published edition: a beautiful type of woman, aristocratic because naturally perfect. My aunt Emma was unmarried.

I was put at a dame-school close to St. Thomas's Church at Ryde, managed by the Misses Cotterell, where, like Sir Walter Scott, I was in a class of two—"me and a lassie." There I made friends with two boys, Alfred Dashwood and another youngster, two years my junior, Frank Aston-Binns, the son of a Baptist pastor, who lived opposite St. Thomas's Church at Ryde. Mrs. Aston-Binns was another second mother to me, and Frank's elder sister Mary used to manage us boys. Mr. Aston-Binns came of Quaker stock, and the whole family was highly cultivated. Mrs. Dashwood was also very kind to me.

In 1866 my uncle Wilmot moved to 2 Lansdown Villas, a sunny little house in the garden of which I planted a sycamore tree, which I found fully grown when we visited Ryde in 1908—I grew it from the seed. By that time I had learned to read easily, and the first books after the Bible I ever read were the Elizabethan dramatists—Shakespeare, Massinger, Chapman, and Marlow; and I was so imbued with these works that I remember my uncle exclaiming one day, "Why, the boy talks Elizabethan English!" I read also *Don Quixote*, *Robinson*

Crusoe, Pope's Homer, Milton, and Hume's *Essays*—all in my uncle's library. I liked the *Iliad*, Massinger, *Dr. Fausus*, and *King Lear* the best. I had a secret passion for music, but was always ashamed to sing. I also discovered perpetual motion. The idea was quite simple. In a model four-wheeled carriage I connected the front wheels with the hind wheels by a system of cogs taken from a clock, and argued that if the hind wheels were set in motion the cog-connection would force the front wheels to go round, which would then in turn necessarily and similarly keep the hind wheels moving; and my disappointment was extreme when I found by experiment that my reasonable expectations were not realised. I took great interest in my aunt Emma's aquarium, containing sea-anemones, crabs, barnacles, and minnows; and we boys used to ramble in the Quorr Woods, sail boats at low tide, bathe in the sea off the piers, fish for "whiting pout," or shoot at birds with catapults. We dined in the middle of the day, and at 6 p.m. the tea-urn would hiss, and we supped on bread-and-butter, sardines, and eggs; and my aunt Harriet read books aloud afterwards. We had prayers twice a day. A society of good, kind, and cultured people, perhaps the best in the world. I do not remember a single jar, quarrel, or storm.

One year I spent several months in summer with my aunt and uncle Barwell, who had bought a small property called High Cross Grange, near Lutterworth in Leicestershire, where I saw something of farming and of the habits of cattle, birds, frogs, newts, and wasps.

In 1867 my mother returned home with my sister Marion and my little brothers Claye and Charles, and took a house at Ryde, where I joined them. Shortly afterwards my father also came back, and we moved to Beech Cottage, Dover Street. In 1868 (I think) my parents and myself spent two delightful summer months at Lowbridge, in Westmorland, the house of my mother's aunt, Mrs. Fothergill—where I learnt fishing and some more natural history. At about this time I was made a day-boarder at a school at Ryde. I remember little of these times. During the holidays my friends Aston-Binns, Lowder, and Dashwood and I used to forgather again and enjoy ourselves with boats and catapults the whole of the broad summer days. The woods, sands, sea at Ryde, the two piers, the old quarantine hulks anchored off shore, the opposite shores of Hampshire, the great men-of-war in the Solent, the distant towers of Osborne, and the gorgeous sunsets are fixed in my mind. I frequently visited the parents of Dashwood and

Aston-Binns, who always made me welcome. I spent one holiday with the former at Totland Bay; and on anothre occasion Gerald Lowder and I made a walking tour in three days round the Isle of Wight.

In 1869, when my mother was returning to India, I was sent to a boarding-school at Springhill, near Southampton, kept by the Rev. Mr. Carrick. I preferred football to cricket, because the reactions of my hand and eye are not quick enough for any ball-games. On the other hand, I was a good walker, and loved rambles, especially on the beautiful Southampton Common. Springhill also possessed a delightful garden, and I was given a plot for myself with a small cucumber-glasshouse, in which I kept lizards and frogs, and an occasional snake. I collected stamps and butterflies, and used to grow the latter from the caterpillar. At that time I was very zoological and commenced definitely to write a book which should contain a description of every known species of animal—being ignorant of the fact that there are some millions of species. I collected my data out of my uncle Wilmot's Cuvier and Buffon and from the Rev. Henry Wood's books on natural history. My own great book was an illustrated one which commenced with the apes; but it reached only as far as the monkeys, bats, and cats —it is still in my possession. My pleasure in animals was largely associated with a wistful curiosity regarding the countries in which they live, and I always possessed a desire for the sunshine, blue skies, and warm air in which I had spent early childhood. The cold of England did not suit me very well, and I suffered much from nasal catarrhs and broken chilblains, though I was never seriously ill. I was neither popular nor unpopular at school, and scarcely remember any of my school-fellows, except Alfred Dashwood.

Regarding Latin and Greek, the former always annoyed me intensely because of the unfortunate habit which the Romans possessed of putting a noun and its adjective at different ends of a sentence—a very unreasonable practice; but, in spite of the abominable English pronunciation, I enjoyed the music both of the *Iliad* and the *Georgics*, through which I stumbled along heavily. Horace was always an almost hopeless puzzle to me, though I greatly admired his terse beauty and tried to understand his measures. Regarding mathematics, Euclid was amazingly incomprehensible to me until I came to Book I, Prop. 36, when his meaning suddenly flashed upon me, with the result that he no longer presented any difficulty whatever. I became very good at geometry and liked to solve problems

for myself, and remember that I solved one in my sleep during the early morning. Algebra also interested me; but the teaching of all these things was, as usual, atrocious, our time being wasted over grammar and small particular problems of multiplication, division, and equations. I was taught drawing and painting, and also glee-singing and the playing of mazurkas by rote upon the piano. I learnt much more about painting by watching my father make his admirable water-colour drawings; and in 1873 I was bracketed first in all England for drawing at the Oxford and Cambridge Local Examination—my success being due to a pencil copy of Raphael's Torch-bearer, which, as I remember perfectly, was done in a few minutes and was almost exact. In music I was much interested in working out for myself the elementary laws of harmony, and was soon able to harmonise any melody in any key; but I was slow at reading music, and my teachers never showed me any good music except hymns. My father and mother sang duets very well, but my own voice was never trained. In English I learnt grammar and parsing very easily, but ridiculed the teaching by means of extracts from Shakespeare and Milton; and we boys were never given a single classical work to read through so as to understand the meaning of it—the instruction was merely philological. The money which my parents spent on my "education" was largely wasted, and I could have learnt as much in a few months by being given the run of any small library, such as my uncle Wilmot's. But I was not really keen to do anything, and was therefore probably an uninteresting pupil. My mind was mostly engrossed with dreams, chiefly of natural objects—many gorgeously coloured visions of sunrises and sunsets, golden cities, galleons on rolling seas, monarchs on thrones, mighty warriors and great victories, forests full of animals and birds, depths of ocean, and savages on prairies. I did things merely to pass the time, and absolutely without any conscious desire to become accomplished in any line. Oddly enough, I remember my thoughts more clearly than my experiences.

While my parents were in England I saw something of them during the holidays. My father was, of course, on furlough, and knew a number of soldier friends in Ryde who were at home on similar leave. He used to spend the morning painting landscapes in water-colours, and treated the same Indian themes over and over again in order to perfect his results; but each attempt occupied him only for a day or two. His ideals were broad lights and deep shadows with rich colouring. The

Indian plains and Kashmir and the Himalayas occupied him incessantly, and he worshipped sunrise and sunset. The snowy mountains glowed above rich slopes of forest and placid levels of lake. His work never seemed to interest his friends, though I believe it was unique in many ways, and he was certainly a poet in water-colour. I remember that he once tried to sell some of his best pictures at a picture-shop, just to see what the public thought of them. In 1870 he and my mother, with some of the younger children, returned to India.

Most of my holidays were spent at Ryde with the Wilmots, or with the Aston-Binns. One delightful holiday I spent at the house of my uncle, Captain Edward Elderton, J.P., at Knochboyne, Navan, County Meath, in Ireland, where he was adjutant of the militia. I remember him as a stout, florid, cheerful man who had acquired something of a brogue and was very popular with everyone. His eldest son, Alan, was my chum—a handsome, spirited boy, who afterwards entered the Army and died in India. My younger cousins were Mabel, Frederick, Ferdinand, and Violet; the eldest daughter, Kathleen, having been drowned long before when bathing. Their mother was dead, but my aunt, Marion Elderton, kept house for my uncle. They were a delightful and kindly family, and the servants, Tim and Joe Kerrigan, I remember very clearly. We used to fish in the Boyne and to ramble all day in the woods. My uncle had a steam launch in which he was wont to take us up and down the river, even as far as Drogheda; but as he snored heavily sleep at night in the small cabin was almost impossible. On one occasion we ran short of food and were almost ravenous when we arrived at a village beyond Drogheda. Alas, there was nothing to eat there but cockles and new bread-and-butter—of which we partook so generously that I, at least, suffered agonies for some days afterwards. The altercations at the canal locks, and between the Captain and one of the Kerrigans who managed the engines of the launch, were always highly amusing, and the society of the local gentry was not less so.

I remember my grandfather, Edward Merrick Elderton, vaguely as a rather tall, dark, spare man with "mutton-chop" whiskers turned grey. He once lived in a fine house in Pimlico, where his third wife (*née* Miss Cameron) gave large parties at which I saw people reciting from *Macbeth* or moving and talking as described by Thackeray. Afterwards they often visited us at Ryde and at Southsea, and our step-grandmother

was always something of a great lady, besides being a very good one. He died in 1875 and she much later.

When I was at Southampton my father's brother, Charles Edward Ross, was the medical officer of one of the great ships of the Peninsular and Oriental Steamship Company, which berthed at that port. He always came to see me, tipped me, and took me away for pleasure trips. I had a great affection for him; but my father was dissatisfied with him because he did not attempt to enter the higher walks of the profession. He was a blond, handsome, dreamy-looking man, with an affection for cigars, and was of course unmarried. Once he fell overboard in the Red Sea and was rescued only after two hours' immersion. In 1872 he brought me home a live chameleon, which I kept for months in one of Mr. Carrick's hot-houses at Springhill. I fed it on flies and moths, and thus described it in a letter to my mother:

"People say it *cannot* change colour completely. I have *seen* it change from bright green to black and again to blue. . . . There are four very peculiar things in it—the changing of its colour, the length of its tongue and the queer formation of its eyes and feet. I have told you about the first. It lives on flies and other insects that fly off at the least thing. To catch these it must move either very slowly so as not to startle them, or very quickly, more quickly than they. When it sees a fly it crawls up very slowly towards it until it is within reach of its tongue, which can easily stretch four inches, although the whole of it is about six; it then gets ready its tongue, which it shoots out like lightning and sticks the fly on to the tip of it. The eyes are large, being the size of a large pea, but they are covered, all but the pupil, with skin. It can look at the tip of its tail with one and with the other can see what's going on with respect to flies at the tip of its nose. Then again his feet are very funny indeed. The chameleon is made to climb trees and to crawl along the branches. Its feet are made like two human hands joined at the wrists, and are made so that he can easily grasp a twig parallel with his body. Claye and Charlie are quite well and are much better in their manners."

These were my brothers, who were also at Springhill at that time. Alas, my uncle Charles died on one of his voyages two or three years later; and some of his scanty property, his medical and surgical books and instruments, and his fine

gold watch (a Chronomètre de Poche, by Long & Padoux, Geneva, No. 1819) were given to me when I went to St. Bartholomew's Hospital. The books were lent to friends and, of course, never returned. I wore the watch (which kept perfect time) until 1917, when it was lost after we were torpedoed in the *Château Rénault* off Ithaca (page 522). My chameleon died in the winter of 1872 because the old fool of a gardener "watered" it for a joke when watering his flowers in the greenhouse, in order to refresh it, he said; I am still angry with him!

During all my schooldays I had suffered in general health and development, I expect, from my illness when I was a child in India; and I was rather ill from pleurisy during one winter at Springhill. I was dilatory, "moony," and slow in growth —somewhat like the young Frederick the Great, I fancy, whom his father kicked downstairs. But I improved, I hope, as I grew older.

Early in 1873 my mother and then my father (who was now in command of the 14th Sikhs in India) returned home again and took a house at Southsea, where my sister Marion, my brothers Claye and Charles, and myself joined them during the holidays, and with several younger members of the family must have made the house a pandemonium. My father had many military friends there (including John Tytler), and used to take me out for walks with them—when I learnt much about life in India. Every morning after breakfast he turned to his painting and treated the same Indian landscapes over and over again in order to perfect them. At meals he told tales of battles, fishing, and shooting, described vividly whole scenes from Shakespeare, and rolled out passages from Byron:

> Mont Blanc is the monarch of mountains,
> They crowned him long ago
> On a throne of rocks, in a robe of clouds,
> With a diadem of snow.

To him Byron was the genius, and Shelley, Keats, and Tennyson only more careful and scholarly imitators. He delighted in Scott and Coleridge, thought Swinburne an academic genius, but disliked Wordsworth for his bathos. He knew little of Homer and the classics, but revelled in the great structural art of the Shakespearian dramas—so often lost upon modern critics. Early in 1875 his two years' furlough was exhausted and he was obliged to return once more, extremely loth, to his last turn of Indian service, leaving his wife and children behind him.

I remember that during my last year at Springhill School (1873–4) I began to be moved towards the writing of painful verse. My efforts were strictly confidential and were inscribed in pencil upon the backs of old letters and in decayed note-books; but I made experiments on various kinds of metre, fitted words to songs and songs to words, and even commenced certain melancholy tales in verse. My first lyrics were full of despair; and my first epic was the story of a dreamy ineffectual called Edgar, beloved by a gentle angel who, however, was ultimately carried off by one Julian—though I never reached this distressing end; and my somewhat feminine muse dealt much with the woes of the gentle Œnone. I abandoned zoology and stamps, but continued water-colour. The boys at Springhill enjoyed much wholesome sea-bathing in the summer, and I delighted in swimming, boating, sea-fishing, and sailing, and on a fine morning at Southsea swam out nearly to one of the forts islanded in the Solent, and back. My friends at Ryde I had gradually lost touch of, but I visited my dear aunt Harriet occasionally for sympathy and advice. Aston-Binns was at a public school (Rugby, I think) and was developing a brilliant capacity for modern languages. I much regret that we were not put through military training at that age—a grave defect in British education.

So in 1874 I left beautiful Springhill School, with its lovely gardens, meadows, and brook, and its gorgeous summers, and scarcely saw the place again until the spring of 1918, when I inspected the camps on Southampton Common for the War Office while the American troops were passing through, 5,000 of them every day. They arrived without a sound in the dead of night, and left next day about noon—fine, straight fellows, but not so cheerful, I thought, as Mr. Atkins. Night after night the same thing—tramp, tramp, tramp. This ends the war, I concluded. . . . Springhill was then being used as a Government office; it was as beautiful as ever, but half its grounds had been alienated.

CHAPTER III

THE COMEDY OF YOUTH. HOME, 1874–1881

My age was now seventeen years, and it was time for me to choose a profession. I wished to be an artist, but my father was opposed to this. I wished also to enter the Army or Navy; but my father had set his heart upon my joining the medical profession and, finally, the Indian Medical Service, which was then well paid and possessed many good appointments; and, as I was a dreamy boy not too well inclined towards uninteresting mental exertion, I resigned myself to this scheme, especially because it would give me experience of life in India, with shooting and riding, and also a knowledge of biology and considerable leisure for any other hobbies I might have a mind for. But I had no predilection at all for medicine and, like most youths, felt disposed to look down upon it.

It must have been in 1874 that my father called upon the Warden of the College attached to St. Bartholomew's Hospital in London (Dr. Norman Moore) to ask his advice as to whether it was necessary for me to go to Oxford or Cambridge before entering the hospital. He was told that it was not necessary—which is to be regretted, as I would probably have "found my feet" sooner at a university without costing my parents much more money. Consequently on 1 October 1874 my father himself delivered me at the Hospital; and I was given a bedroom and a sitting-room numbered A3 in the College, which was rather a mean building looking out upon a mean street appropriately called Little Britain. I was not happy that day.

During the first year we studied chiefly anatomy and physiology. The usual preliminary dislike of dissection soon wore off, but I took little interest in anatomy because it is only a kind of geography of the body—to be learnt by rote. But physiology, histology, and microscope work engaged me more, though still in a far-away manner, because they invoked problems of causation and the wonders of the body's mechanism.

I made a number of friends at St. Bartholomew's. George Dennys was the son of an Indian Colonel who was the very incarnation of Thackeray's Colonel Newcome; I stayed with the family for a few days at Reading in my third or fourth year of studentship. Charles Bamber formed a trio with Dennys and me. We lived in contiguous rooms in College and were always together; but Dennys and Bamber worked much harder than I did and became good students really interested in medicine—which I never was; and both entered the Indian Medical Service later on, but, owing to their hard work, went to the northern parts of India, where the good candidates go. Dennys was musical, sang, danced, played the piccolo, and was a courtly devotee of fair ladies; and Bamber was an admirer of Macaulay and of great statesmen and orators, and possessed a stature of soul much greater than that of his body. Another friend, McKee, was lean, strong, and like an eagle; and his peculiarity was that he was extremely taciturn and sarcastic all day long but became suddenly jocose precisely at midnight, when he signalled his sudden change of humour by throwing a shoe or a book at someone and challenging him to a wrestling match. He also joined the Indian Medical Service and died in it—I was with him in Madras about 1883. Other friends were Sylvester, a gentle studious soul, and Nicholson, who had been partly brought up in France. We used to have study-parties and wine-parties; and of course I learnt to smoke, a habit which I have never been able to conquer.

Apart from men such as these, there was a much coarser stratum of society at the Hospital, which, taken together with the dullness of the autumn and winter in London, did not cheer me at all. But the work was strenuous, and we had no time for melancholy or for the riding of hobby-horses. At the end of the first year, however, my father having departed to India, my mother took a house for the long vacation of 1875 in a pretty village (the name of which I cannot remember) some miles north of the Portsmouth Downs in Hampshire. The summer was lovely; and we knew some relatives of Martin Tupper, the poet (a better poet than the critics allow), and also a young literary man, with whom I could discuss such far-away things as poetry and music. Here at last, as I remember, I began to feel the sprouting of the seed of endeavour within me: I would take seriously to verse. My natural bent was no less than epic-drama; and I proposed to find for it a rolling and jewelled surge of rhythm hitherto undreamt of in English,

and set myself to study euphony to begin with. For this purpose the miserable "Edgar" was recast into Spenserian measures, and "Œnone" was developed into a little drama—not at all for publication (I published nothing till fifteen years later), but for the sensuous pleasure of the verse. I also wrote several lyrics, two of which, "Recession" and "Hesperus," were printed, little changed, in phonetic spelling in my *Lyra Modulata* [115] thirty-six years later, the others being still unborn. Mr. Masefield liked "Hesperus" so much that he translated it into vulgar spelling and recommended it for the first volume of *Georgian Poetry* (1912), from which it has been copied into the anthologies *Northern Numbers* (Second Series, Edinburgh, 1921) and *Later English Poems* (Toronto, 1922). It was there that I first read Shelley and Keats, but rather slighted them; Tennyson, Browning, and Swinburne I did not know; I thought that the Romanticists were only *revenants* of the Elizabethans, especially Spenser, and of Milton; and much preferred the classicists to what we called the Methodist School. I was bold enough to aim at once at perfections, especially as regards word-music.

After this vacation (1875) my mother and sister Marion went to India to join my father, who was then appointed Brigadier-General in command of Peshawur and who gave me a handsome allowance. Our work now consisted chiefly, so far as I can remember, of chemistry, botany, and *materia medica*; but I spent much of my leisure in studying music. I hired a piano, and, though I remembered little of what I had learnt of music at school, I was audacious enough to set myself down to master Mozart's and Beethoven's sonatas, not to mention Mendelssohn. This I attempted with pleasure, if not with success, and at the expense of my neighbours and, probably, of my medical studies. Presently I learnt by heart the whole of Mozart's *Phantasia* and of Beethoven's *Moonlight*, *Sunrise*, and *Pathetic* Sonatas, for I have always found it easier to learn music by heart than to read it. What interested me chiefly was to investigate and to master from these musicians the general principles of harmony and, above all, the great laws of form—which distinguish high art from low and which my father always studied so carefully in his sketches. Obviously the same laws of form appertain to all great arts, from the *Iliad* and the pediments of the Parthenon to Shakespeare, Massinger, Raphael, and Beethoven, and these must be studied together if they are to be studied effectively at all. I also practised the flute upon an old one which had belonged

to my grandfather Elderton. At the same time I tried my hand at composing music—lyrics, sonatas, and part-songs—and I still possess a number of fragments which I courageously headed Op. 1, Op. 2, etc.; fragments, because they were never completed so as to satisfy my taste. I seldom went to concerts, but I remember seeing Wagner conduct extracts from his then much criticised works at the Albert Hall, where I believe they were first adequately performed in England in 1877.

My friend, Frank Aston-Binns, had been threatened with phthisis at school and was ordered to go south, and as his father had then come in to some money and had joined the Church of England and given up his Baptist ministry at Ryde, the whole family migrated to the Continent for some years. About 1877 they returned to England. I found my friend now to be a rather tall thin youth, with aquiline nose, reddish hair and moustache, a carefully disciplined stammer, a diplomatic laugh, pronounced French manners, a large bow necktie, and a green umbrella. He scoffed at my having entered the medical profession, and was over-polite to my college friends; but he imparted to me his large experiences in Italy, Switzerland, and France, where he had made several aristocratic friends. He had great aptitude for languages—Greek, Latin, Italian, French, German; was a well-trained pianist, well read in general literature, and educated in all the arts, thanks to his admirable parents. We forgathered again in London, and he taught me especially the beauty of Italian painting at the National Gallery, and read me poems in Italian and French so that I could catch the music of them. He was depressed at times on account of his lungs, and I remember, though I was only a student and the treatment was not advocated at the time, that I urged him to take to mountain climbing in Switzerland (for which he had an opportunity) as a cure. He did so, became quite cured and a very bold and capable climber.

I think that I must have visited my uncle, Lieut.-Colonel William Alexander Ross, Bengal Artillery, at his house in Shepherd's Bush during my first year at the hospital, but certainly saw much of him during the second year (page 7). Just about that time his wife, Henrietta, died suddenly, and he was left with a number of children and an infant to look after. His eldest daughter was a tall handsome girl, and then came a son younger than myself; but it was himself and his wit and enthusiasm for science that drew me to his house and gave me an impulse towards investigation. I bought the outfit advised by him for blowpipe chemistry, and could be seen in

my rooms puffing out my cheeks before the flame in emulation of him, but in a manner which provoked some ridicule from my friends. A little later he married again and I lost touch with him owing to my absence from London. My uncle, Charles Ross, also died about that time (page 26).

Of course the medical staff of St Bartholomew's were the greatest physicians and surgeons of the age. I remember the admirable lectures of Sir James Paget—a lean, gentle, intellectual man ; and also the stimulating influence of Mr. Thomas Smith, the witty and talkative surgeon, who was something like my uncle William. Dr. Callender was a somewhat dull lecturer on medicine, but cheered us up occasionally. The good students sat upon the front benches near him and the bad ones far back, where they made a noise. " Ah," he exclaimed, " I have always heard that civilisation spreads from a centre outwards." On another occasion he was describing the colour of a " large white kidney," while a number of the back-benchers were reposing with their boots on the benches before them. " In fact, gentlemen," he said, " the colour of a large white kidney is exactly the same as the colour of the soles of those gentlemen's boots." But the hero of the hospital, the great Napoleon of Surgeons as we thought him, was Mr. Savory, with his fine head, keen but somewhat cynical face, and accomplished dramatic oratory, of whom someone had written on a wall :

> Great Savory of Bartholomew,
> By the nine gods he swore
> That, of five-and-twenty candidates,
> He'd pluck twenty-four.

I was given an introduction to him and became a " dresser " under him ; and I was a " clinical clerk " to Dr. Callender. In the latter office I was put in charge of my first case of malaria, a tall fierce woman who had caught the infection in Essex—one of the last of such cases which occurred before the recent war. I was interested in the case and questioned her so minutely as to her symptoms that she thought something was wrong, became angry, and left the Hospital " statim " ! —like the man who, according to the Poet Laureate, fled from hospital when he saw the words " ter die " written on his bedhead ticket.

One day (I think it was in the winter 1875–6) we had a most delightful riot. A student threw a snowball at another, missed him, and hit a policeman outside the gates of the hospital, full in the face. The enraged constable entered to

seize the culprit ; other students defended him ; other policemen entered in support, until there were about thirty of them. But they found themselves in a nest of hornets, for we numbered some 300 or more ; and one by one the good-humoured limbs of the law were overpowered and thrown out of the hospital gates, which were slammed to after them by the laughing janitors. Unfortunately I did not hear the commotion at first, and thus missed most of the fun—and also the punishments which followed. One red-haired policeman, whom we nicknamed Ginger, lost his temper and suffered badly in consequence.

The long vacation of 1876 was spent by me with my cousins at Navan (page 25). Alas, my uncle, Captain Edward Elderton, had died suddenly on 15 January 1875, at the Grosvenor Hotel in London, but my orphaned cousins still lived at Knockboyne in charge of our aunt, Miss Marion Elderton, who devoted her life to them. My cousin Alan was about to enter the Army through the Meath Militia, and looked very fine in his new uniform and chaffed me for being " a mere doctor." We bought canoes and made long excursions in the summer weather upon the Boyne. One day we went several miles out to sea at the mouth of the Boyne ; and then took a delightful trip of ten days up the Blackwater, a tributary of the Boyne, to the small lake called Virginia Lake. I carried our tent and Alan carried the cooking-pots. The August weather was heavenly, and the peasantry, who had never seen canoes before, ran after us along the banks and gave us eggs and milk *gratis* —the dear kindly souls. They were haymaking, and the round sun set on one side of the river and the round moon rose on the other ; and we camped on an island in the lake, where the owls hooted all night.

> Give báck to me, O time-forgetting Náture,
> The dáys of my bóyhood ;
> Give báck to me those gólden days of súmmer-time,
> To wísdom so wásted but wásted not to life.

Alan preferred cooking to poetry—so perhaps does the reader.

I think it was next winter (1876–7) that I returned to Navan in order to study pharmacy with the local doctor, whose name I have forgotten. My cousins were away, and I lived somewhere in the town for a few months and saw some " general practice "—a dull time.

Next year my friend Dennys suggested that I should learn a great deal if I could obtain the post of unqualified Assistant House Surgeon at Shrewsbury Infirmary, which he himself

had filled for some months. I obtained the post, and was in Shrewsbury during the winter 1877–8, and did obtain invaluable clinical experience and practice. The great event was the arrival of a new house surgeon shortly after my own arrival—a Dr. Collins, I think—who came bursting with the revolutionary surgical doctrine of Mr. Joseph Lister (afterwards Lord Lister, P.R.S.). We were somewhat sceptical; and of the four surgeons to the Infirmary, two were opposed, one was dubious, and one, a pushing little Welshman, was favourable. Collins, a pupil of Lister's, was an enthusiast for the doctrine. I was converted by facts in a week or two. Previously to his arrival patients suffered much pain in their wounds after operation, and I was almost always ordered to give morphia injections, while many cases went wrong and wounds healed slowly. Carbolic acid (we used spray in those days), cleanliness, and carbolised gauze dressing wrought a miracle—no morphia, and healing by "first intention." I enjoyed the life here, and had friends in the town, especially some people called Haycock, whose son was a student at Bartholomew's and became a chum of mine. But I must confess that the medical profession and all its associates and associations were little to my taste or inclination, though I appreciated the magnificence of medical discoveries, such as that of Lister. During my stay at Shrewsbury (? six months) I did some painting and sketching, especially an idealised sketch of Shrewsbury Bridge as (not) seen from the Infirmary.

Before I went to Shrewsbury, Dennys and I had left our rooms at the College at St. Bartholomew's Hospital and had taken rooms in Percy Street. When I returned from Shrewsbury to these room, Dennys had gone somewhere else, but I made friends with a number of choice spirits of a type less studious than my former associates. H. E. Haycock was a good-looking youth, whose calm expression of face belied his frequent vivacity of mind. He also performed on the piano, and we annoyed the neighbours alternately with stumbling andantes from the classics—each reading medicine or surgery while the other was more pleasantly engaged. Then there was a little sprightly man called H—— or D——, who was full of music-hall humour and who borrowed money of me freely. There was a fat young man called P——, who was always our superior in wisdom; and numerous others of similar types. The life was that of Falstaff, Prince Hal, and their friends, and there were plenty of humours of all kinds. I remember once that we took a box at a music-hall, so situated that the

trombone player was placed just beneath us, and that H—— slowly poured a glass of beer into the large mouth of the trombone. The German performer was enraged, the audience were delighted, and we were expelled. On another occasion, when the orchestra was tuning its instruments, one of our party (which was seated in the front row of the stalls) caused amusement by demanding of the conductor in a loud voice: "What opera of Wagner are you performing, sir?" Racing men were not among us; but we did much boating on the Thames, were fierce critics of new plays (I saw Wagner, Phelps, Irving, and most of the stars), discussed philosophies, with beer and tobacco, and experienced "life." Later on, Haycock and I moved into rooms near the Angel at Islington, where, I remember, we frequently dined on Welsh rarebit and stout —a diet which would now be fatal. Lectures were much neglected, of course, but we took more interest in the out-patient and midwifery courses, and in operations; and my experiences at Shrewsbury had already given me considerable clinical knowledge and practice.

Behind all this my mind was growing very slowly in the soil of experience, though I was now over twenty years old. At first the growth was quite unconscious, like that of a plant, and I looked upon medical experiences as the shard of a seed, meant merely to be thrown aside in the process of further growth. By degrees, however, I formed for myself a definite though audacious, programme: that I should seek every possible experience and try my hand at every possible art; and I remember ruling for myself that I should not attempt to form conclusions or publish results until I was at least forty years old—which seems an extreme age to a boy. I continued attempts at musical composition, and also tried moulding in clay and plaster of Paris—especially a small figure of Prometheus on the rock. My attempts at verse I kept secret from my associates, who would have ridiculed them. After the beginning mentioned on page 30 I conceived the idea of writing short type-dramas of one scene each (of which I now have a number), and began with one on the death of Paris, in which Œnone on Ida is watching the burning of Troy. Suddenly wounded Paris comes to her and demands to be cured: she hesitates, and he dies in dithyrambics. Another drametta gives the picture of two Greek lovers in a wood. They quarrel, when Pan, Silenus, and their train enter (with some comic business) and compose the difference. I also polished previous verses, and wrote a ballad, "The Night-ride." The latter is

accused of being like the "Erl-king," but is really quite different—written some years before I read Goethe (though I may have known Schubert's song). In fact, I read very little, and my small library consisted only of some British poets and medical books. The following verses express the detached, if not indifferent, attitude of the medical student of those days:

COMPULSION

No more in languid idleness I lie
And let the easy hours run rank to waste,
Praying my Muse, who doth me ever fly,
Supremely chaste;
Or watching my poor hand the pencil ply
In faulty taste;

No more with fingers roaming o'er the keys,
To soothe the sadness I myself have made,
I wake sweet music whose rich harmonies
By fancy sway'd
Steep all the soul in bliss: or, tired of these,
Think on some maid.

No more with book on knee and wandering eyes
I sit at ease and easy thoughts combine
Of fates and daggers, kings and wizards wise,
And poisoned wine;
No more, no more such idleness—I rise.
'Tis time to dine.

But a more exigent compulsion fell upon me in 1879. My father, who was then still commanding at Peshawur, was anxious that I should become qualified at once in order to enter the Indian Medical Service before he retired, and it became necessary for me to undergo examination by the College of Surgeons for the membership. I procrastinated in reading, up to within three days (actually) of the examination. Then, fairly alarmed, I went to my friend McKee, who had been "grinding" for the same examination for months past, and asked him to "coach" me. He was scornful but amused, and did so. We were examined together, and I passed while he failed! He was very angry. That was on 30 July 1879.

But it was necessary to have a medical qualification, and the easiest to take at the time was that of licentiate of the Society of Apothecaries in London. Then came my punishment. Having obtained the M.R.C.S. with only three days' reading, I decided to read for the L.S.A. only on the morning of the examination (commencing, I think, at 3 p.m.). But there was no time to go through the poisons and their antidotes. The *viva voce* commenced; but the examiner took me—on poisons and antidotes! I failed.

I was not allowed to try again for some months, so that I missed my chance of competing for the Indian Medical Service in 1879. This has always lain very heavily on my conscience, because I could easily have passed the examination, or indeed that of the College of Physicians, if I had really tried. Of course my father was much annoyed, and threatened to reduce my allowance. I determined that it was not fair in me to continue to take it; and, as a single qualification sufficed for surgeoncies on board ships, I went to an agent and, by dint of a little bribe (called an extra fee), was appointed surgeon of the s.s. *Alsatia*, of the Anchor Line, a moderate-sized vessel running between London and New York.

My original intention was to take only one or two voyages in order to read properly for my medical qualification, but I found the life so pleasant and instructive that I took, I think, four or five voyages (it was no longer possible for me to reach India before my father's forthcoming retirement). The ship carried, besides cargo, first-class passengers both ways, emigrants to New York, and sometimes cattle to London—so that the society was agreeably varied; while I was an excellent sailor. The officers were nearly all broad Scotsmen, sober aboard, but expansive ashore, and I made friends with all of them, especially the captain. The saloon passengers were not always of the best classes—decayed American "millionaires" with drug-habits, reverend gentlemen of various persuasions, doubtful ladies, emigrating gentlefolk, bumptious Westerners, and so on, who all insisted on talking to the doctor. I remember especially on the homeward voyage two sprightly young French ladies, who had been brought up in the States and talked American with the horrible twang of those days; while everyone else on board had some "accent" or other. At the end of the voyage one of these ladies said to me, "Now doctor, do tell us; your accent is much softer than everyone else's on board this boat; say, what is it?" I replied, "Well, the truth is that I am the only one on board this ship who speaks English!" My English was what the phoneticians call "educated Cockney"; that is, with h's, but without r's.

The emigrants, poor people, afforded the most interesting study. We had several consignments of Irish and of extremely dirty and unhealthy German or Polish Jews crowded together in the after decks, so that, as Thackeray said, when the ship rolled:

> All the fleas in Jewry
> Got up and bit like fury.

It was my duty to go round among these poor people and try to mitigate the horrors of their sea-sickness. The sights seen are not capable of description in print, except by a Swift. I found laudanum and perchloride of iron the best medicine (then known). The scenes round the surgery door were worthy of a Hogarth. One day a Russian complained of the quality of the ship's butter, and asked for some castor-oil instead! There were several births and deaths on board, and one small epidemic of typhus. The captain was always on tenterhooks regarding what I might tell port medical officers, but there was no trouble. On one occasion, to make amends for the Jews, a large party of Norwegian peasantry came on board—the finest people I have ever seen, tall, blond, *débonaire*, clean, and without a rent in their clothing—who spent the day waltzing on the hatches over the holds during the glorious weather. On another voyage we were chased from Newfoundland to the Channel by the most appalling north-wester, raising seas of three waves to the horizon, as it seemed. The extraordinary characters and opinions which I saw and heard were a revelation, and the ship was an epitome of human life. Later, about 1883, I commenced a novel called *The Emigrants*, describing the things I witnessed; but I never finished it.

One day we were hailed by a tramp steamer and asked to send a surgeon on board for an operation. There was a heavy sea running, but the second officer and I got aboard and found that one of the engineers of the tramp had had an arm nearly severed by the engines while he was oiling them. From above the elbow downwards the arm was entirely mortified; and I was obliged to amputate still higher up, while the ship checked, rolled, and wallowed, and the officers, kind and handy men, gave what help they could with the chloroform, the arteries, and the ligatures. But we were too late, and I heard subsequently that the poor man died after all.

During my abundant leisure I worked in my little surgery, a side cabin on deck, for my examination, and also at my verses. I wrote a whole Spanish drama, called "Isabella," in blank verse, and commenced a drametta on Ajax and Cassandra, and a novel called *The Major*, illustrating my experiences as a medical student—also not proceeded with. I was gradually forming a lofty philosophy of utilitarianism, according to my lines written later:

> Cannot the mind that made the engine make
> A nobler life than this?

and an equally lofty theory of poetry. Unfortunately the world

has no ears for either: but the young man labouring there between wind and surge in that shipload of humanity had as yet no knowledge of this fact.

About 1879 my father was transferred from the command of the Peshawur district to that of Calcutta, and in 1880 he retired with the rank of Major-General and Knight Commander of the Bath (military). Towards the end of 1880 my mother and sister Marion, and then my father, returned home for good and took a nice house near Southampton, where I joined them after my voyages in order to work for my examination. I passed for the L.S.A. early in 1881, and then for the Indian Medical Service, but in the latter took only the seventeenth place. My commission as Surgeon in the I.M.S. was dated 2 April, when I went to the Royal Victoria Hospital and School at Netley for the prescribed course of military medicine and surgery, lasting four months.

The young officers of the Indian and the Army Medical Services were taught together at Netley, and we enjoyed ourselves immensely. We were quartered (I in a cottage) close to the hospital and to the beautiful ruins of Netley Abbey. Broad open lawns extend between the Hospital and the sparkling Solent, with Calshott Castle and the Isle of Wight in the background. The summer was a gorgeous one, and I heard the nightingale every night in my quarters, and was also badly bitten by mosquitoes. We played lawn-tennis and boated on the sea, and were very proud of our uniform—and of ourselves. Surgeon-General Maclean, a fine-looking old man and a dramatic speaker, gave us excellent lectures on tropical medicine, especially on the treatment of malaria and dysentery, and Colonel Notter taught us well on sanitation, water and food analysis, ventilation, etc.; but we were told nothing of bacteriology, which was then an established science on the Continent; nor of the parasites of malaria, which had just been discovered by Alphonse Laveran, a French Army surgeon, in Algeria (page 119); and indeed reading in recent medical literature and parasitology were not properly enforced. I worked hard, hoping to better my place in the concluding examination so as to have a choice of presidencies in India—I wanted to go to Bengal; but I actually went down two places. At the end of the course we all had about six weeks' leave before proceeding to India.

During the course my sister Marion was married (25 August) at Southampton to Mr. George Thomas, a Calcutta merchant, a fine man and secretary of the Calcutta Gymkhana. I was

" best man," and my father gave a ball which a number of my fellow students at Netley attended. All my brothers and sisters lived with my parents. My brother Claye was then just entering the Indian Army, and spent the August holiday in making a small balloon and a canvas boat with me—both were failures. My father was thoroughly enjoying his retirement, and spent the morning painting beautiful copies of Indian sketches. I emulated him, but attempted more ambitious subjects, such as the rebellious spirits being hurled out of heaven, and the doomed in hell watching the far-off vision of heaven—both of which I still possess (I regret to say). The technicalities of sculpture and painting are more complicated than those of poetry, or indeed of musical composition, and of course the human figure alone cannot be mastered without years of apprenticeship even by those who are acquainted with anatomy. But I made a series of experiments in colouring, especially on the effect of coloured " glazes " in oil on a black-and-white under-painting, as indicated by certain half-finished old Italian paintings. At this time also I wrote out or rewrote a number of my attempts at musical composition, especially music to several of Shelley's smaller lyrics, the two " Laments," " Night," " Time," and " Tell me, thou Star," and added accompaniments to two songs by my father, " Stars of the Summer Night " (Longfellow) and " The Rose upon the Balcony " (Thackeray), which gave him great pleasure. But my accompaniments, like my sonatas, were beyond my executive ability, and my only interpreter was my sister.

On 22 September 1881 I said good-bye to my parents and brothers and sisters and embarked on board the great troopship *Jumna* for India—and for the serious business of life. The voyage lasted a month. I remember little of it except the Suez Canal and the heat in the Red Sea, aggravated by a gentle following breeze. The ship was crowded, and we were forced to wear thick English uniform of red or blue cloth and were not even allowed to unbutton our tunics. I was the only man on board who wore an upstanding collar, because I had taken the precaution to buy a celluloid one ! The men were packed like sardines in unventilated lower decks, and one remarked when we inspected them, " This is wot I calls 'ell."

CHAPTER IV

THE MORNING STARS. MADRAS, BANGALORE, 1881–1885.

We arrived at Bombay on 23 October 1881, and I remember being wakened up at the hotel next morning by the cawing of a crow which was calmly seated on the rail at the foot of my bed examining me with one eye—nothing unusual for Indian crows, but a bad omen for me if I had been superstitious. I went by train (three days) to Madras, and was there posted to the Station Hospital (for British troops only), as there was then no vacancy in the Indian regiments for the newly arrived young doctors; and in fact we found later that promotion was blocked for us for some years by the incompetence of someone who had overcrowded the junior ranks. The work done at the Station Hospital was very ordinary, and required only a couple of hours in the morning and an occasional visit in the evening. The senior medical officer was a rough but kind Irishman, and he and the other officers were pleasant enough. I found that I now had the leisure and independence for which I had entered the Indian Medical Service.

I lived at the Dent's Garden Hotel, some distance from the Hospital—a large old house, with great lofty rooms, plastered walls, deep verandas, cool bathrooms, swarms of mosquitoes day and night, endless cawing of crows and squeaking of squirrels, all characteristic of India. Immediately on my arrival, possession had been taken of me by my servant, Appoo, who adhered to me till he died while I was on furlough about 1894, and who was a thin, good-looking man, capable and honest, except for the usual servants' perquisites, and I owe his memory considerable gratitude. After a little time I obtained an excellent suite of rooms right at the top of the north face of the hotel, overlooking the hotel "compound" (or grounds) and the large houses and trees of Madras from a considerable height. I worked all day in the cool breeze in the veranda, where the sun seldom entered, and soon passed

(9 January 1882) the necessary lower standard examination in Hindustani (which I had utterly forgotten).

As with other young people, the change of climate to the broad sunlight and air of India filled me at first with an extraordinary vigour, and, after the morning attendance at hospital I laboured all day at various pursuits. I never needed special exercise, because I often spent hours pacing up and down my veranda thinking out problems of all sorts and devising future works. Messrs. Higginbotham's book-shop provided me with literature; and I started on a thorough course of the world's poets, bought Italian, French, and German grammars and dictionaries in order to master them, and, with the help of translations, learnt at least something of Tasso, Alfieri, Racine, Hugo, Schiller, and Goethe, besides trying Homer, Æschylus, and Virgil again. There was scarcely anyone in the hotel except a venerable old Armenian gentleman called Aretoom, so that I had most of the day to myself. I used to wake before dawn and leap out of bed and look from my lofty veranda at the morning stars in an extreme exaltation of spirit; there they flashed and flashed, before the sloped sword of the zodiacal light smote them and the dawn dispelled them. Then the city also awoke, the wayfarer trilled his song in the road, the strains of the military bands at early parade floated in the air [1]; and then, alas! the sun and the crows came also. That was "my watch-tower in the east" mentioned in the first verses which I wrote in India. I lived really in solitude; but it was a time of integration of thoughts and experiences, spent with the high gods themselves.

Europeans who visit India for the first time are always much struck by the character of the people. As hard-working as any, faithful, docile, and intelligent, yet in many parts they possess amazingly delicate physique combined with great timidity and a habit of unquestioning obedience; and the swarming millions of them are generally very poor and ill-fed. What is the cause? Is it the climate or the diseases connected with the climate, or have we here merely the picture of an old civilisation fallen into decay—what every civilisation falls to, what China has fallen to and what, probably, the old Roman civilisation fell to? If so, will the vigorous populations of Europe also sink some day to the same level, or can science find a way to prevent this? I wrote:

[1] I remember chiefly the old and pretty tune called "The Punjab March," by C. Payne, which we hear played to-day in London by soldiers in the streets (1922).

Here from my lonely watch-tower of the East
An ancient race outworn I see—
With dread, my own dear distant Country, lest
The same fate fall on thee.

Lo here the iron winter of curst caste
Has made men into things that creep ;
The leprous beggars totter trembling past ;
The baser sultans sleep.

Not for a thousand years has Freedom's cry
The stillness of this horror cleaved,
But as of old the hopeless millions die,
That yet have never lived.

Man has no leisure but to snatch and eat,
Who should have been a god on earth ;
The lean ones cry ; the fat ones curse and beat ;
And wealth but weakens worth.

O Heaven, shall man rebelling never take
From Fate what she denies, his bliss ?
Cannot the mind that made the engine make
A nobler life than this ?

The theory here is that men can better society and themselves and banish most of the ills which now afflict them by precisely the same scientific methods as they now employ in the making of their wonderful engines : and the rest of my life has always been attuned to this note.

About February or March 1882 I was put in medical charge for a few weeks of the 10th Madras Infantry, which was in camp near the Mysore railway terminus, to the west of the large station of Bangalore, where I served later on. This station is 2,700 feet above sea-level and has an Italian climate. My vigour was such that every morning I was up and out for a ramble at dawn in time to see the " morning stars singing together." Then I returned for hospital, bath, and breakfast, and went out with sandwiches and note-books in my pocket for a day-long ramble westward over the swelling slopes of the country, then in stubble. Seldom did the *sahib-log* do such a thing except when out shooting ; and the poor Indian peasants and large-eyed cattle and black swine and scurrying birds and evil crows and swooping kites stared at me as I leaped along. I found rocky crests of mountains, fir woods, dells with beautiful little trickles of streamlets—all in miniature, and the sun and the cool breeze were those not of earth but of heaven. I thought only of these things, and have utterly forgotten every other thing and person I met there—only the scenery and my own thoughts remain in my memory. About ten years later I endeavoured to find my fairy summits and

glens again ; but this part of the country had been somewhat built over and they had vanished.

During these rambles I formed some of the impressions of Indian rural life which I tried much later to fix in my fragment of the "Indian Shepherds" (page 290)—especially the concluding fragment of the cowboy singing before dawn—a thing I heard from my tent pitched at the end of the line, though, of course, I did not understand his song. It was also then that, seated on the rocks which crop out of the soil in places, I wrote the whole of the first draft of my "Judgement of Tithonus"—a drametta. Old and blind Tithonus comes out to the sea-shore before dawn to await the coming Aurora. Presently she and her train arrive, bringing (innocently) Orion, whom they have found sleeping. He awakes and they talk blankversely, when suddenly Diana arrives in a fury at the loss of her friend and accuses the Morning of impropriety. They contend in words, and Tithonus is called upon to judge between the two goddesses, without knowing their names. He awards Orion to his own wife. Diana discloses the fact that she is his wife, and the decrepit Tithonus rages. Eos, however, comforts him with her fidelity, and all ends well among choruses :

> They that are shepherds do rejoice
> And they that hunters are.

Meanwhile I had long been engaged upon a much more serious effort. Before leaving England I had seen Gilbert's *Pygmalion and Galatea*, in which the latter, after coming to life, finds herself the unwitting rival of Pygmalion's wife and therefore turns again voluntarily into marble. This climax appeared to be sleek with the sentimentality of the time, though really our witty modern Ben Jonson was aiming the shaft of satire at British mediocrity-worship. I thought that this was poor game and that Apollo should not waste his darts upon asses. The benign ancient fable was evidently a parable of the Art Spirit, probably invented by some forgotten poet long before the days of writing ; but the question was what actually happened after the marble had so miraculously turned into flesh. I conceived a very ambitious epic-drama indeed, with quite another climax, and built it out of the stone quarried for my "Edgar." Like *Job* and *Faust*, it begins with a "Prologue in Heaven" ; for I held (with Goethe) that an artist may borrow whatever he pleases if the perfection which he is designing requires it. Heaven opens, and Jehovah, surrounded by choiring angels, utters his deeds. But presently sounds of

tribulation are heard from the infernal, and Satan challenges the Scheme of Things and brings up before Heaven the spirit of a man to bear witness for him. The spirit, dazed with brightness, gives an ambiguous answer; and then Satan wagers that even if the man be granted all that he asks yet will he die lamenting. Christ accepts the wager, Heaven closes, and the dreamer returns to earth. Then the drama commences. The time is not classical, but mediæval—and (openly) Gilbertian. Prince Amaralza is a fantastic patron of the arts and a poet-aster himself, surrounded by poets and critics strangely resembling those of 1880! His nephew, Edgar, is a dreamy amateur sculptor, who has spent his youth in the creation of a perfect statue, which he calls Niobelle. In the dead of night he prays Heaven that it shall come to life, and it does so. But the living Niobelle (who has forgotten her origin) develops a character averse to that of her visionary creator and enters with joy into the trivial pleasures of Amaralza's court. Edgar rages in solitude; and then, one night when the tragedy has developed, persuades her to enter his studio once more and implores Heaven to turn her back into marble. So, dragging her dying lover Julian at her feet, she is drawn irresistibly to the dreadful pedestal upon which she was carved out of stone and returns to that substance. Nevertheless, Satan is duly refuted in the epilogue. All three versions of the legend are true, but I fear that mine is the commonest.

So far as I remember, the first drafts of both these poems were completed in Madras early in 1882, but were laid by when I went for a few months to Vizianagram. When I returned I revised them, and as I then became much engrossed in mathematics, I determined to print them in order to get them "off the mind," but always with a view to amending them greatly some day—which will probably never arrive. They were printed at my cost by Messrs. Higginbotham & Co., 165 Mount Road, Madras, in 1883. I believe that no one ever bought a copy, and I never saw a review. I still have a few copies, the residue having been "wasted." I commenced a rescript about ten years later, but did not proceed with it, though my chorus—

> O Vísion invíolate, O Spléndour supérnal,
> We stánd in thy whíte light like lámps alit in dáy . . .

in *Philosophies* was meant for the new prologue. I sent a copy recently to a distinguished literary friend, who wrote me on 30 May 1917 that *Edgar, or the New Pygmalion*, "seems to me, at a rough glance, to have the material

for half a dozen plays, and to contain some extraordinarily good verse." There is also some extraordinarily bad verse—quite like " the New Poetry," in fact !

On return to Madras from Bangalore in March 1882 I was appointed to the acting medical charge of the 17th Madras Infantry for six months. It was then stationed at a little place called Vizianagram, about sixteen miles inland from Vizagapatam, on the east coast of the Indian peninsula some distance north of Madras (two or three days by steamer). It was a pretty station, almost dried up when I arrived, but presently converted into a garden by the rains. There I became acquainted with Indian cantonment life—the young Indian civilian and his newly married wife, the few officers of the regiment and their ladies, the young Maharaja—all pleasant people, doing a few hours' work a day, and then playing tennis or billiards at the mess, or going out shooting. A Captain Butler and his wife were particularly kind to me, and he instructed me in the arts of the sportsman—though I could never become a good shot owing to my slow co-ordination of eye and hand. We went out on elephants several times after leopards, but without seeing one ; but duck and ground game were plentiful. The early start by the zodiacal light, the dawn, the "cooing of innumerable doves," the scented morning, the breakfast under the tree, the long beat in the blazing sun, the luncheon and rest, the beat home, and the dinner at mess—better than the home life of a professional man in England. The Maharaja entertained us frequently, and on one occasion I remember that his jester, seated on the ground, emitted loud eructations beside him, much to my horror, until I learnt that this was an Indian compliment to the patron, denoting receipt of sufficient hospitality ! Though many of the *sahib-log* quarrelled with each other, I made friends with all and enjoyed life greatly.

As a rule, after an hour in the morning at my hospital and an occasional patient outside, I had the day to myself, and worked at poetry and prose, seated in a " long-armed chair," with a low table beside me or a board to write upon across me. I put away *Edgar*, so that I might be able to examine it more critically later after forgetting it, but tried several other things. I commenced a number of fables in four-foot couplets, in which many delicate points of art were attempted, combined with that common wisdom which we all possess—and ignore. The fable and the fairy tale—most joyous, most natural, and yet most immortal of all the forms of art—probably

invented before the cave-men figured their habitations, æons before writing was invented, by the seers and poets who must have lived even then, and handed down from parents to children to guide us among the thorns of life, do they not live in our souls to-day ? For the race, like the individual, forgets the thousand realities of everyday life but remembers the dreams. What else are the mythologies of Greece, Scandinavia, Egypt, and India, and the folk-lore of all nations ; and indeed all the supreme masterpieces from the *Iliad* to the *Idyls*—summative, philosophical, metaphorical, and utterly distinct from the journalistic literature of the real and the sordid, writ only for the day. . . . In my own humble efforts each was to contain a core of poetry and drama enveloped in a carefully careless style in which the elision, the broken sequence, and even the bad rhyme were threads purposely woven in. I had already conceived some of them, but now wrote out with delight " Ariel and the Hippopotamus," " Puck and the Crocodile," " The Frog, the Fairy, and the Moon," " Orpheus and the Busy Ones," " The Contest of Birds," and others. They were frequently amended afterwards ; and I have added many from time to time and still possess a number lying in my brain, like chrysalids, each in its cocoon, awaiting a day of emergence which will probably never come ! I made no attempt to publish any of them for twenty-five years, but when some of them were brought out [113] that scrupulous critic, the late Mr. Dixon Scott, said of the style : " It is distinguished, it is deft, it is always rich, it is often beautiful, and the veins of its orthodox figures are filled with colours so fine that they become wholly vital and romantic "—what, indeed, I tried to make it—but perhaps the reader will disagree ! In " The Poet and the Penman " I described the immortal combat between these adversaries at the foot of Parnassus ; and the picture of the mountain which I formed there in India in 1882 was exactly what I saw it to be from the Copaïc Plain in 1906—though I am not a clairvoyant ! (page 495).

At the same time I wrote as an experiment a whole comedy called *The Misanthropes*, which is Elizabethan in character —plot, shadow-plot, blank-verse soliloquies, etc.—but deals with modern life. I fancy that some of the scenes were amusing —rather like *Charley's Aunt*—but I have read the thing only once again since I wrote it, and would not like to give it yet another trial. I also continued or revived two novels, *The Major*, describing the life of medical students, and *The Emigrants*, describing my experiences on the *Alsatia*. The

former was to be a comedy of character. The latter proposed to run the emigrant ship upon an ice-floe ; in the end two actors in an illicit love-drama who had been separated for years were to be thrown together upon a small berg, alone except for the presence of a beautiful English-Norwegian mad girl (a character I had actually seen in the *Alsatia*), while the ice was gradually to melt. Both stories were interrupted by the " calamity " shortly to be described, and I have never touched them again nor even read what I had written. I wrote also a weird short story called *The Vivisector Vivisected*, and an essay on Wordsworth, whose ideals I have always thought wrong. More than this, I began to block out, as every young man does, my own general scheme of philosophy—a mechanistic one, though I fully grasped the failure of the human intellect in face of infinities.

I returned to Madras and to my old quarters there in September 1882, and resumed duty at the Station Hospital, there being no permanent vacancy for me as medical officer of a native regiment (by which I lost Rs.100 a month). It was about then that what I call my great calamity occurred. One day I commenced to read an old prize book which I had won at Springhill School for mathematics in 1871, called *The Orbs of Heaven*, by O. M. Mitchell (Routledge), which eloquently described the great mathematical triumphs of the astronomers ; and I was so fired by the theme that I determined then and there to study mathematics. I bought nearly all Todhunter's series at Higginbotham's book-shop and, literally, went through them in a few weeks, up to the end of the Calculus of Variations, though I had not advanced beyond Quadratic Equations at school. Of course the positions had to be consolidated later with greater labour ; but it was remarkable with what ease the mind of the youth of twenty-five mastered matter which that of the boy found so difficult, even when taught by practised masters, and the fact is that education must be chiefly self-education, during or after school, or it will never approach completion at all.

I can scarcely describe my enthusiasm. It was an æsthetic as well as an intellectual enthusiasm. A proved proposition was like a perfectly balanced picture. An infinite series died away into the future like the long-drawn variations of a sonata ; and the Binomial Theorem or Integral Calculus was like the crash of the *Iliad* or of the so-called *Sonata Pathétique* ! The æsthetic sense is indeed largely intellectual satisfaction at perfection achieved ; but I saw also future perfection to be

achieved by the potent weapon of pure reason. The stars of the evening and of the dawn (when I rose to go to hospital)—Orion, Aldebaran, and, above all, Sirius, Canopus, and the Centaur—were now doubly beautiful since they had been caught in the net of analysis. I soon began to read the applications of mathematics to motion, heat, electricity, and the atomic theory of gases ; and remember thinking from the first of their possible application for explaining why epidemics of disease exist, on which Miss H. P. Hudson and I have recently published papers [96]. But I was always impatient of reading mathematics, and felt that I should like to have created the propositions myself ; and indeed new propositions suggested themselves while I was reading the old ones. Directly I returned from hospital to my " watch-tower " at Dent's Gardens Hotel and had my breakfast, I plunged into the garden of figures and laboured not only all day, but in my sleeping, and often woke with a solution which could not be found overnight.

I have forgotten how long this first rage lasted, but I remember that suddenly I began to feel weary, and what I had never before known—stale. The fact was that the vigour of youth which, if kept in proper channels, might have served to fertilise really valuable work, was being poured out (like the Bolán River) to run to waste and be absorbed in infinite sands. Mathematics were quite useless to me, either as a medical man or as a writer. If I had discovered my love for them at the age of fifteen or so it would have been different, but what could I do with them at the age of twenty-five ? It was the unfortunate passion of a married man for some beautiful but inaccessible lady.

Some time early in 1883 I became so exhausted that I laid aside my figures—though I knew it was only for a time—and returned to poetry. I commenced several pieces and wrote out especially my favourite fable, " The Poet's Retirement," which I give here because it is a bit of biography. Of course the poet is idealised ; and some lines are amended, especially one unconsciously borrowed from Tennyson.

THE POET'S RETIREMENT

Down from that blithe Idalian Hill
Where Violets drink of dew their fill,
And wading thro' wet eastern flowers
With wash'd feet Eos and the Hours
Come laughing down, I laughing came.

The Morn had now her threads of flame
Enlaid to Earth's green tapestries,

Gold-inwoven ; and to their knees
In chilly baths of thridding rills
At tremble stood luce Daffodils ;
When lo I mark'd toward me move
Those Maidens Three whom poets love.

" O whither away, glad Youth," they cried,
" Singing thro' daffodils dost thou stride ? "
" Ladies I wander for a while "—
And here I duck'd and doff'd in style—
" I wander by Bourn, I wander by Byre,
By Cape and Cote and Castl'd Spire ;
Or sometime stick in puddled Mire,
Or climb the summits of Snow and Fire.
Or where the hoarse moon-madden'd Tides
Drench dripping jags on Mountain sides ;
Or twanging strings sound gay reprieve
To smoky Villages at eve,
What time the paddock'd Ass careers
Mirthful, with high-prickt tail and ears,
And slow towards their wattled home
The baaing Sheep do go, I roam.
And I have left behind me there
Hippocrates teaching the air ;
And Learning prim ; and Venus too
Now whipping Cupid with her shoe."

Then, of those slipper'd Maidens, She
Robed in flush rose-red answer'd me,
Who brightly gazing with mild look
Held still a finger-parted book.
" Come then," she cried, " with me and dwell
In my Valley of Asphodel,
Which is a land of laughing rills
And hung about with dazzling hills,
Where oft the Swain with garter'd legs
Piping for love in music begs,
Nor Thisbe turns her petulant ear.
There large-eyed Plato thou may'st hear
Persuade, or, if not idly awed,
Masters a Master's theme applaud.
And then if Thunder more invite
Than silver-threaded rain's delight
And sloping seats of knollèd moss,
Come where some thwarted Torrent toss
Thro' cloven Gorges, mad to shake
The shagreen'd Boulders black and break
The gleaming silence of the Lake.
Or, if engross'd with human Fate,
On rangèd boards mark Love and Hate
Egg on to midnight-living crime,
And glaring Horrors of dead time
Creep in behind. Or, restive still,
Unlock'd from Hell soar Heaven's hill
Thro' sun-outstaring Cherubim."

" Not so," cried one, a Virgin slim,
Plumed, wrapp'd, and robed in the gold-green
Thro' sunset-dazèd woodlands seen ;
Who half upon her dinted breast

Apollo sculpt in little prest.
" Come to my House of all delights,
Whose marble Stairs with mergèd flights
Are shallow'd in the viewless Lake ;
Whose overpeering Turrets take
The peep of Dawn, or flashing turn
To Eve departing golden scorn.
There fairy-fluted Pillars soar
To cloudy Roofs of limnèd lore,
And Walls are window'd with rare 'scapes
And rich designs ; of blazon'd Capes
Pawing the sunset-burnish'd flood ;
Of rib-rail'd reaches of Solitude ;
Of rounded World and globèd Skies,
And Stars between, and faint Moonrise ;
Of black Tarns set mid mountain peaks
And spouting silver-foamèd leaks ;
Of Gods reclined, and Maids who move,
Unlidding lustrous eyes of love ;
Of War ; of Wisdom with a skull.
And in the high aisles Fountains full
Disperse a stream of coolness there
For frosted fern and maidenhair,
And sculptured Beauty holds the way.
So thither go with me to-day."

Then She who all in purple dight,
Brow-star'd with orbèd ruby light,
Lifted from under rich deep locks
Looks wrapt on Heaven, to earthly shocks
Descending, thus replied : " Not these
Flat hapless lands of Towers and Trees
May past the morn your spirit please.
But to some cold Crag, that doth lift
His brow to Heav'n above the drift,
And turns beneath the mistless Stars,
Come. There no dew distilling mars
With felon fog or frozen haze
The many-hued Sidereal blaze,
Where Planets pale not age to age,
And moonèd Venus in white rage
Stares down the Dawn. Come ; for that Glow
There solves to unpolluted flow
The crumbling crystals of the Snow ;
And windworn Cataracts wavering plumb
To lightless pine-valleys. Come, O come !
Lest those faint Harmonies be unheard
Which, as from strings of silver stir'd
By the light fingers of the Wind,
Run from the poised orbs swiftly spin'd."

She ceased, and with her finger tip
Made sound the lyre upon her hip,
And would have sung ; but I replied,
" To be unchosen is descried ;
And we shall be made mad in Heaven
By all the divers good things given.
I love all Three so passing well
Which I love best I cannot tell.
Alas "—I cried, but checked the word,

For close behind a footstep heard
Compell'd me turn ; when lo that Maid,
Dress'd in black velvet, who bewray'd
Plump Popes and Pastors once to fear,
Came up and took me by the ear.
" Is this the way," she cried, " you waste
Time should be spent in huddling haste
To harry Ignorance to her den,
Or pink fat Folly with the pen ?
Small unobservèd things to use,
Each with its little mite of news,
To build that sheer hypothesis
Whose base on righteous Reason is,
Whose point among the Stars. For shame !
Enough the seeming-serious game.
But search the Depths ; and for thy meed,
A place among the Men Indeed."

Then also I decided to print my " Edgar " and " The Judgement of Tithonus," as I have said. In this I was largely guided by looking through a copy of Bailey's *Festus*, which someone lent me, which is something like " Edgar," and which seemed to have been spoilt (I have never read the book properly) by a life-time's tinkering. But before this was done I already had another mathematical fit and was too impatient to correct a number of gross flaws, such as the poor opening of " Edgar." Higginbothams suggested that they might try to sell some copies, but I never heard whether they did so. I then sent a copy to my father in England, threw the remainder into a box, and lost all interest in the work. Urania carried me off, and the other Muses were completely forgotten for the time.

Among smaller matters which interested me in moments of relaxation at Vizianagram and Madras was shorthand writing, of which I devised several systems. This led me to consider phonetic spelling, which I then began to employ for my verses, in order to study English rhythm and euphony.

With the printing of " Edgar " and the onset of the mathematical furor ended what I consider to have been the first phase of my self-education. That was in 1883, when I was twenty-six years of age. For the next five years all my thoughts and leisure were given to various mathematical studies, with various lucid intervals of rest devoted to studies in verse, prose, and music. But I gradually saw more of the outer world, and engaged in pleasant sports.

Later in 1883 I was obliged to leave my " watch-tower in the east " at Dent's Gardens Hotel and to occupy Government quarters in Fort St. George. Madras is built upon an absolutely flat coast cut away by the sea in a straight line running north and south. A dull, stagnant stream or ditch called the Comb

enters, or rather does not enter, the sea at one point, and here the old fort, surrounded with a high wall and moat, is built, and contains barracks for a British regiment, offices for the military staff, mess, etc. My new quarters were in the middle of the fort, and were not nearly so pleasant as my former rooms —heat, mosquitoes, and the incessant noise of drilling, of children squalling, of servants quarrelling, of crows and kites, etc., to which I added the strains of a piano which I now bought as a solace. The Station Hospital was situated some little distance outside the fort, and there I went every morning at 7 a.m. for the most ordinary medical work on slight cases, lasting an hour or two. My friend McKee (who had coached me for the M.R.C.S. and was as thin and aquiline as before) also had quarters in the fort, and we messed together and not at the regimental mess. In the evening we hired a brougham, visited the hospital if necessary, and then drove along the fashion-crowded beach, listened to the band, or strolled away farther to watch the moon or stars rising in the east before dinner, and to catch what cool air we could. On one occasion I got what McKee said was cholera, but what might have been toxin poisoning.

This life lasted, I think, nearly a year, during which we continued to lose Rs.100 a month owing to the fact that there were no vacancies for us as medical officers of Indian regiments —not our fault, and an example of the usual unsatisfactory administration which had obtained our services on very exaggerated prospectuses. But, lost in my numerous visions, I was quite indifferent to such a small matter, and found my pay quite sufficient. We had, of course, called on the Governor and chief officials, and knew a few ladies; but we did not trouble about society, and as we played no games and (unlike most of the wholesome young men in Madras) did not even ride, society did not trouble about us. What men I knew seemed to take interest in absolutely nothing, and I have forgotten everyone except one or two pawky doctors who studied chiefly the Madras Army List for chances of advancement—*vide* my sonnet in " Philosophies " [114]. Even McKee took no interest in any of my numerous projects and experiments, derided my mathematics, and ridiculed all poetry, of course !

At that time my scientific studies were becoming concentrated round a single very difficult theorem. I had long accepted as a possibly useful working hypothesis in ontology the view that there are only three entities, Space, Matter, and

Motion—somewhat like what is now called the mechanistic philosophy. Matter occupies parts of space and motion occupies matter and changes its relations to space. Time is only the ratio of motion to space. All forces—light, electricity, heat—are only manifestations of one elementary force, motion; and all substances consist of one elementary kind of matter differently disposed in space or differently affected by molecular motions. Mind is, of course, a kind of mechanical work and the result of the interplay of matter and motion in space, and the living thing is merely an engine. We can make no pretence to understand "the things in themselves"—space, matter, or motion; and infinities are beyond our minds —for we are always "up against" the contradiction that everything is limited and yet there is always something beyond anything, both in space and time. But, putting aside this breakdown of the reason, the theorem of Mechanical Trinity, as I called it, seemed to me a likely one owing to its simplicity.

But if motion be the only force, how explain various kinds of attraction between bodies? I supposed that really these attractions are only apparent and are due to the shelter afforded by one body to another against a universal bombardment of particles coming from all directions from without. Thus two large masses bombarded by gaseous molecules coming from all directions would shut off from each other molecules moving in the straight line joining their centres, so that the masses would tend to be pushed together with something like the laws of gravity.[1] This would imply bombardments of equal momentum passing through all equal planes facing in all directions, and, secondly, in the case of heavenly bodies, vortices of small particles rotating in the same direction as the bodies, besides being affected with the bombarding movements first required. In order to try to explain cohesion on this tentative hypothesis I set myself the following proposition: Suppose that in a space full of equally bombarding particles from all directions there came by chance a mass of molecules touching each other, or in close proximity. Then an inelastic particle bombarding the mass from without would, on impact with it, lose its movement and join the mass; but this movement would travel through the mass from molecule to molecule and would finally tend to drive away from the mass some of the molecules situated on the other side of the mass in the line of motion of the original impinging particle. Thus the mass would gain one particle on one side and would lose several on the opposite

[1] Something like Newton's explanation, though I did not know it then.

side; but these would generally have a lower velocity than the original impinging particle. What would be the final result under bombardments from all sides? Would the mass ultimately break up and be dispersed, or conversely would it constantly grow in volume by the absorption of impinging particles? Take, for example, a mass in two dimensions only, and suppose that a large collection of billiard-balls on an infinite billiard-table is bombarded by other balls from all directions: would the mass certainly always be ultimately broken up? The answer might depend on the average velocity and the frequency of the bombarding balls and on the elasticity of all the balls. If the mass is not always broken up, we might obtain an explanation of cohesion and of the existence of masses of matter limited in space without necessarily invoking any theory of attractive cohesion—that is, by inertia alone.

At any rate, with something like Quixotic courage, I now attempted a mathematical resolution of the problem, if only for its mathematical interest, and worked at it off and on until I returned to England in 1888. I soon found myself in the presence of difficult integrations and of appalling series, which I could not master with the limited literature at my disposal. I tried to obtain help from several mathematicians in India whom I approached; but they could not be persuaded to attend to the matter. I worked mightily and learned much mathematics; but on the other hand I wasted both time and energy, which might have been better spent, both for others and for myself, and, like the impinging particle, I fear I lost over these problems a great part of the momentum of youth which I brought with me. I have forgotten my work now, and have never dared to attack the problems again since my retirement from India. Electric theories of molecular physics have advanced so much that such speculations as mine would probably no longer repay even tentative exploration. I cannot say.

The strain of this severe work already began to be felt at the end of 1883. By that time McKee received an appointment away from Madras, and I lived a thoroughly solitary and unwholesome life, immersed in my problems and regardless of everything else. Fortunately I was rescued from this in August 1884 by my medical duties—when I was sent from Madras to act temporarily in medical charge of the pensioners' depot at Pallaveram, a little south of Madras. That was a station where there lived a certain number of British officers and warrant officers who had elected to remain in India after they were pensioned. Many of them kept their families with

them—with the usual result. There was considerable interesting practice, chiefly in the treatment of chronic Indian complaints; and the pleasant society of many of the people at Pallaveram, together with rambles among some low hills in the neighbourhood, did me much good. But after a few weeks there I was given the Acting Garrison-Surgeoncy (for six months from 6 September 1883) at Bangalore, probably the best station in Southern India. I had been there already for a few weeks in 1882 (page 44).

The large city of Bangalore is situated in the territory of the Maharaja of Mysore (whose capital is Mysore); but close to the city of Bangalore there is a considerable area which contains the civil and military station of Bangalore, and is within direct British jurisdiction under the British Resident at Mysore and the general commanding the troops in the station. These troops included both British and native cavalry, artillery, and infantry; a large garrison, whose barracks were separated by distances of several miles over the area. In the centre there was a large maidán (plain) for parades, games, and riding; to the north there was an extensive golf course (near which I lived later on (page 91)); and to the south-east lay a very considerable native town of about 90,000 inhabitants and the lake called the Ulsoor Tank. The whole place is situated on a large gently undulating plateau, extending for miles in every direction, with clusters of boulders and broken rocks scattered about here and there on the tops of the slopes. As the plateau is about 2,700 feet above sea-level, the climate is almost Italian in character—fairly hot and dry in March, April, and May, much cooler with the early rains in May, not over hot nor very wet during the big rains, and with a delightful, clear and cool winter. The ground is green most of the year. There are the Cubbon Park, the beautiful Lal Bagh Garden, the Club, endless games, and a pleasant military social life. Outside the native town and the various barracks, the station consists of numbers of villas, situated in gardens, some of which are very pretty and well-kept. Unfortunately, for some mysterious reason, the climate did not suit everyone (including myself), and there was a considerable amount of sickness such as typhoid and bowel complaints, though practically no malaria. A picture of pleasure remains in my mind: the sun, the gardens and pillared villas, the cleanly dressed natives, and the open-air, aristocratic life of the British—as goodly a place, probably, as there is in the world.

My duties consisted in the medical charge of the General

and his staff, of a number of pensioners, of other details not included in regimental units, and of several small hospitals and dispensaries. I succeeded a Dr. Bain, whose pony-trap, pony (and syce) I bought for 600 rupees. The pony was an exceedingly fast-trotting and well-behaved Pegu pony; and I spent my mornings scampering along all over the station visiting my patients, in whom I took great interest. I messed at first with the 4th Madras Pioneers, stationed close to the Maidán, and lived with the adjutant. That was the first time that I became interested in mosquitoes. They devoured me in our bungalow until I discovered that they were breeding in a tub just outside my window, when I rid myself of nearly all of them simply by upsetting the tub. When I told the adjutant of this miracle, however, and pointed out that the Mess House could be rid of mosquitoes in the same way (they were breeding in the garden tubs, the tins under the dining-table, and even the flower-vases), much to my surprise he was very scornful and refused to allow me to deal with them; for, he said, it would be upsetting the order of nature, and, as mosquitoes were created for some purpose it was our duty to bear with them! I argued in vain that the same thesis would apply to bugs and fleas, and that according to him it was our duty to go about in a verminous condition! I did not know then that this type of fool is very common indeed, and I suffered much (not gladly) from the type later on. Subsequently I moved into another small house facing the Maidán, and there installed my piano from Madras.

I made many friends, was busy all day, and enjoyed the work and the society, but still had time to continue some mathematical work. I laboured much at trying to reduce algebra and the Calculus to a series of dependent propositions in the Euclidian form, and still think that this is after all the best method for record and teaching, as is proved by the fact that Euclid still remains the master textbook on geometry, in spite of all efforts to oust it. These studies of mine soon led to my work on "operative algebra" (pages 64, 493). Also I had found in Madras a copy of the *Introduction to Quaternions*, by P. Kelland and P. G. Tait, and had soon grasped this most beautiful system of geometry ever invented (by Sir William Rowan Hamilton). But now it occurred to me that it might be rendered even more perfect by the adoption of another interpretation of the product of two "vectors." Hamilton laid it down that the product of two vectors normal to each other is a third vector normal to both. To me it seemed that this

offended against the whole idea of multiplication, according to which the order of a product is the sum of the orders of the factors; and I therefore proposed that the product of any two coinitial vectors is more properly the area of the parallelogram between them. This was worked out fully in the following years and was ultimately found to have been already invented by Grassmann (pages 95 and 414); but in 1901 I published a paper [101] showing how the ideal "algebra of space" consists of the systems of Hamilton and Grassmann combined, and I have perfected the matter still further. But life is short!

I saw a good deal of medical practice in this temporary post and enjoyed it; but in the spring of 1884 the much-coveted permanent appointment was given to the son-in-law of the secretary to the Surgeon-General at Madras. Some described this as a job, but I had no complaint to make as I was junior to the new incumbent (the permanent post was given to me later in 1890). I was now transferred to the temporary medical charge of the Queen's Own Sappers and Miners at Bangalore. They had a fine mess and many agreeable officers, of whom I remember Colonel Hamilton, Majors Wilkieson and Goodwin, and Captain Swain, a keen elephant hunter in Somaliland (all of the Royal Engineers). We played lawn tennis in the evening and whist after dinner, at which I had an extraordinary run of luck. Three of us lived together in one bungalow; and when I suddenly determined to learn the violin, my chums elected to follow suit, and we made the house pandemonium because we all practised simultaneously! We did not attempt polo then, and there is not much shooting near Bangalore.

In September 1884 I received orders to go at once to Quetta, in Baluchistan, in order to bring back the 1st Madras Pioneers, who had recently suffered much from sickness on active service against the Baluchis. I enjoyed the long and novel journey immensely—first by train to Bombay, then by an empty steamer, which rolled almost gunwales-under, to Karachi, and lastly by train again through the arid plain of the Indus to Sibi, at the mouth of the Bolán Pass. This was early in October, and the heat was intense. Later I wrote in my "Indian Shepherds" in memory of these scenes:

> Those
> Who drag the peevish camels on dusty march
> In Scindia, where far-foaming Indus toils
> Through sand to the sea—his bosom is full of stars
> For the Deep and his end are near.

So may it be with all of us near the end. I remember especially the last morning of the journey, when we crossed the Indus at Jacobabad. The heat was dreadful, and for miles upon miles the train seemed to be running along an isthmus of sand, with vast lakes or marshes some distance away on both sides. Numerous wooded islets or single trees stood in these endless lakes, with their reflections standing out clearly in the calm water. During a halt I asked a British sergeant who was in the train what he thought the lakes were, and he replied that he thought the train was running between two broad branches of the Indus. But we ran on for hour after hour and still the lakes remained on both sides and the afternoon sun even began to glitter on the placid water! Of course there were no lakes at all; it was only the most perfect mirage imaginable. The desert bushes were, so to speak, cut in half by the layers of air in the hollows of sand, the lower parts of the bushes looking like the reflections of the upper parts.

Suddenly the sun sank behind great dusty mountains as the train ran into the terminus of the railway, Sibi. I spent a week there waiting for a mule-tonga to take me up to the Bolán Pass to Quetta. Sibi is supposed to be the "hottest hole" in India, the temperature often reaching 130° F. in the shade; it was 120° F. while I was there early in October 1884. The place was newly built, with an irrigation canal from the Indus and a station-garden. It was swarming with dogs, which barked, howled, and fought all night long, while we tried to sleep on the roofs and while the native railway employees actually did sleep on the ground, quite peacefully amongst this canine inferno!

I shared the tonga with a man called Lacey, of the Indian Forest Department, I think. It was a two-days' journey up the Pass to Quetta. This was the first time since my childhood that I had seen high mountains. Enormous precipices of pudding-stone soared upward on both sides of the road, which followed the course of the white-pebbled bed of the Bolán River, and we could see the terraces cut in the faces of the precipices by the former beds of the stream æons ago. The heat was dreadful, and the mules had to be flogged the whole way up. At night, however, when we came to the dak-bungalow it was cold. Next day we resumed our journey, and, after passing through a great barren valley full of "dancing dervishes," that is, vortices of dust (of which, I remember, we counted six near us at the same time), we came to Quetta in the evening. I was much interested in the Baluchis we

passed on the way. They were either very handsome " Baluchi gentlemen," riding armed to the teeth on small horses, or else enormously tall shepherds, sheeted from head to heel, with red nose emerging from a fold of the sheet laid across the mouth—also armed and also murderous persons when opportunity offers (we had our revolvers).

It was in this pass that my brother Claye (who had then joined my father's regiment, the 14th Sikhs) had had an adventure a year or so previously. He was riding up the pass with a Captain Rook (I think), and their bullock-cart full of luggage in charge of two poor native coolies of the plain was following slowly after them. Presently they met two gallant Baluchi gentlemen riding down the pass, who saluted them with the courtesy of paladins. Shortly afterwards, however, on looking back, they observed that the cart, some miles behind them on the winding road, had come to a standstill, the bullocks being calmly engaged in browsing by the roadside. My brother and Rook had to ride back to see what had happened. Both the coolies were lying on the ground with their heads cut off ! The Baluchi gentlemen had merely wished to keep their hands in with a little gentle scimitar practice, but had thought it wiser to deal with the weaponless coolies than with two British officers. They had then ridden away into the mountains, doubtless home to dinner.

Quetta itself, which we had recently taken, was a small town of houses, huts, and camps in a flat, comparatively fertile valley, about 6,000 feet above sea-level and surrounded by barren mountains. As the season was then changing, it was very hot, with frequent dust-storms in the day-time, but so cold at night that my Madrasi servant Appoo shivered, in spite of the warm clothes and blankets which I gave him. Owing to good mountain pasturage, the meat, especially the mutton, was the best I had ever eaten in the East. The station was extremely unhealthy, the hospitals being crowded with bad cases of typhoid, dysentery, and malaria.[1] The reason for this appeared to be that the only water-supply was by means of open channels running down from the hills, often by the side of the streets ; and Lacey and I found that one channel ran past the slaughter-house, between pools full of blood and water ! Camp sanitation was not in those days what it has now become. I remember I reported on the matter to someone, but have forgotten the result. I stayed there about ten days,

[1] An account is given in my essay which won the Parke's Memorial Prize in 1895 [16].

waiting for the 1st Pioneer Regiment to return from the front. Before I left, the climate became so cold after sunset that we collected before fires in the Club after evening tennis.

When my regiment arrived I found that one of the officers was a Captain Keary, a school friend of mine at Southampton. We marched by easy stages down the pass to Sibi, but I remember little about the journey. I had brought with me my violin, purchased in Bangalore, which I practised every afternoon after reaching camp—with the result that after each march my tent was placed farther and farther from the others! One day we came to a beautiful bend of the Bolán River, in which Keary, who was a fine sportsman, caught a number of small mahseer with spinning tackle. We spent some days at Sibi, where Keary and I went duck-shooting on the Bolán River, where it sinks into the sand on the plain. By this time even Sibi had an enjoyable climate. Another friend I made in this regiment was Captain, now General, Kennedy.

We met Lord Roberts (then Sir Frederick, and Commander-in-Chief of the Madras Army) on our way home to Bangalore, and he spoke to me very kindly of my father.

I have really forgotten exactly what happened after that, except that I had a very pleasant time. I remained in medical charge of the 1st Pioneers for some months, and was then transferred to Burma.

CHAPTER V

BURMA, THE ANDAMANS, MADRAS, 1885–1888

About January 1885 I received orders to take temporary medical charge of the 9th Madras Infantry (or rather a wing of it), stationed at Port Blair in the beautiful Andaman Islands, situated some 400 miles south-west of Rangoon. The appointment carried with it an additional allowance of Rs.100 a month for charge of a company of British troops at Port Blair; but first I was to take a regiment (I have forgotten which regiment) to Thyetmyoo, near Prome, on the Irawadi River. Selling my charger and pony and trap, and leaving my servant Appoo behind (since he refused to go with me), and my piano in store at Madras, I started off at once with boxes full of books, a gun, a saddle, and much ardour for new experiences. It took us five days to cross the Bay of Bengal in the Indian troopship *Clive* (I think), and then—there was Rangoon, with its many-coloured crowds and its sparkling pagodas and tinkling bells. We camped for a few days in a beautiful wood, then took the night train to Prome, and were in Thyetmyoo after two hours' journey up the Irawadi by steamer. A dull military station I found it; but I was there only for a week, after which I caught the monthly steamer from Rangoon to Port Blair. The voyage lasted thirty hours.

Here I found many pleasant things. The Andamanese Archipelago consists of a string of small rocky islands, richly wooded with primæval jungle and set in the midst of white-foaming and sparkling seas, breaking on skirts of coral, yellow sands, and black boulders, with many calm lagoons between the majestic trees. They are inhabited by an interesting race of diminutive negrettoes—little Apollos, black as jet and not more than 5 feet high, who haunt the impenetrable forests. Port Blair is a fine harbour, with British convict settlements on the shores, containing many hundreds of transported Indian criminals; but the Chief Commissioner, many of the principal settlement officers, and the officers and men of the military

garrison and their wives, live on Ross Island, a large rock some hundred feet high at the mouth of the harbour, in well-made wooden houses surrounded by pretty gardens, with peeps of the sea between the trees. But I was there only for three months, and will return to the subject later in connection with my second visit in 1886. I did much boating and fishing there; went on a week's surveying trip in a Government steamer with Captain Hobday, R.E.; and conceived my idea of Operative Algebra—or Verb-Functions as I called it at first [102, 105].

It was now decided to move the headquarters of the 9th Madras Infantry from Moulmein in Burma to Port Blair; and, as I was a junior medical officer, I was transferred to the other wing of the regiment at Moulmein. I travelled via Calcutta, and arrived there about 1 May 1885, and had the pleasure of staying for a week with my sister Marion and her husband, George Thomas, in their great house, 12 Russell Street. He was the head in India of the firm of Calcutta "merchant-princes" of that name, and was himself an extremely fine man, secretary of the Calcutta Gymkhana, a brilliant polo and cricket player, and shot. Their three little boys were with them; they had many friends and kept an open table; but, alas! a few months later George was carried off suddenly by cholera, in the full ripeness of his manhood. What more is to be said?—except this: Two years previously the great Robert Koch had discovered the cause of cholera in this very city; but, in spite of the fact that cholera kills half a million people annually in India alone, the Indian authorities did scarcely anything to follow up the discovery for ten years (page 184).

I arrived at Moulmein by steamer from Rangoon on 13 May 1885—one of the most beautiful stations in the world. Imagine a great alluvial plain of red earth covered by rich dark green vegetation and cut by four large, slow-moving rivers, crossing each other. Between the rivers there rise many small but rather abrupt hills or mountains, with pagodas glittering on every summit and eminence. The large town lies along the rivers, and the blue Siamese mountains close in the horizon to the south and east. Everywhere the blue sky, the lazy lumbering clouds, with bits of rainbow caught in them, the beautiful trees waving in the breeze, the ringing of innumerable pagoda-bells in the wind, the mingled crowds of Burmese and Indians, the manly British officers and civilians trotting along to parade or polo, and huge timber-lifting elephants trudging slowly to their work.

I lived there for about a year with my friend, Captain Kirkpatrick, adjutant of our wing of the 9th Madras Infantry, in an old bungalow situated on the side of a hill overlooking the town, with the beautiful panorama before us. The medical work was not much, and I spent most of the day in a long-armed chair on the veranda, enjoying the view, hearing the doves cooing, the crows talking, the kites crying, and seeing the light and shadow of the rain-storms moving up from the sea, and toiling at my mathematics. As Kirkpatrick was absent most of the day, I generally had the bungalow to myself, and, as all the other officers of the regiment were married, we messed at home. My faithful Appoo had now rejoined me, and so had my faithful iron-framed piano—all the way from Bangalore and Madras ; and I made thunder-music on the latter when the spirit moved me !

Of course we did many other things. We frequently went snipe-shooting, starting at dawn, and wading all day under a burning sun. As I have often said, I am bad at shooting and ball-games owing to slow reaction between eye and hand ; but I was able to endure the intense labour very well, and enjoyed the sport and the scenery. A Mr. Dodds was our leader in this line ; and other particular friends were Captain Perreau, head of the police, and his charming wife. He was of French extraction and a man of extraordinary vigour and character. He and a Mr. Somerville and others now commenced to play polo on Burmese ponies, and we had fine sport, often playing on the same powerful little animals for three or four chukkurs without changing. One of my ponies was a good polo-pony ; but another, which we called Dot-and-go-One, or Dotty, and which I had bought on Perreau's advice from a cabowner, absolutely refused the polo ground, though he was a splendid young (and small) creature, the fastest trotter and ambler for his size in the station, and such a good jumper that he could take his own height in his trotting stride with me on his back. He was also a most affectionate little creature, and I kept him for years (page 91). We played polo two or three evenings a week, and I was secretary of our polo club ; and every other morning I used to go for long rides on Dotty. In the evening after polo or tennis there was pleasant society at the Station Club, with frequent dancing. For a time all social relations at Moulmein were of the smoothest and kindest, but suddenly the Commissioner's wife, who had been a beauty and who thought herself fashionable, arrived and set everyone by the ears. She started a *clique*, favoured the smart type of women, snubbed the best ladies, and so on. This is a

common phenomenon in India, where the society often sinks to a disagreeable type owing to the domination of " queens of fashion," who are really not the best class of women—*vide* the Indian stories of Rudyard Kipling. They often render the whole life of moderately sized stations quite miserable, making the men follow the quarrels of their wives, and often bullying to an indescribable extent young married women just arrived from England.

Shortly after my arrival at Moulmein I was put in charge of the Civil Hospital there during the absence of the Civil Surgeon on short leave. I found in it a number of old cases on whom surgical operations ought to have been performed long ago, and did the operations at once. When the Civil Surgeon returned, however, he was quite hurt at my activity, though I had cured most of these cases. There was a rich young Mohammedan in the town who had long suffered from apparent epilepsy. His parents applied to the local priest to say which medical man in Moulmein was most likely to cure him. I was told that the priest spun a kind of roulette table and that the pointer indicated my name (or possibly my fame had spread from the Civil Hospital). Anyway, I found that the patient was suffering from a large collection of *Ascaris lumbricoides* in his intestines, and a few doses of santonine and castor-oil cured him completely. This was the second case of the same kind I had met with. My reputation at Moulmein now spread greatly, and I had quite a practice among the Mohammedans.

But the principal event at Moulmein while I was there was that shortly after King Theebaw was deposed in 1885 large bands of his soldiers, whom we called dacoits, came down towards Moulmein and tried to raise a rebellion. Several companies of my regiment went north to meet them, and Perreau ended the affair single-handed by shooting the Bo (leader) as he came dancing down a jungle path to tom-tom music, the remaining dacoits who were not shot by the sepoys running away as fast as they could. But before the news of this victory came south, the British Acting Commissioner,[1] a policeman, and myself, who were following the troops a day's journey behind with a small squad of men, had a very precarious time owing to the threatening aspect of the Burmese

[1] This gentleman tried to march one day on foot dressed in a velveteen coat, a Cardigan waistcoat, a flannel shirt, and a woollen vest, on the theory that these would keep out the sunshine! He collapsed after a mile and had to be carried the remainder of the march.

population. When we joined the regiment, however, we heard that the latter poured out of their villages to welcome the British and the return of peace. I remember that our police had caught a number of Burmese who were supposed to favour the dacoits, and, in order to make them secure for the night, suspended ropes from the branch of a large tree in front of the courthouse at (?) village where we were quartered, in order to tie them to it. I believe that this was a joke of Perreau's, for they all thought they were going to be hanged straight away. But they never changed a muscle of their faces, nor did they show any relief when they were merely tied up to the tree. I think that all of them were released a few days later. The Chinese in this town were robbed or killed by the dacoits and many houses were burnt, so that the people easily recognised the value of British rule.

The Burmese are very fond of an evil-smelling kind of fish-paste called *napée*, and Perreau, who pretended to like everything Burmese, pretended to like this also. One day during the " rebellion " we were floating down a river in a boat when we were struck by a vile odour. " Napée ! " cried Perreau, sniffing the generous air. It was really a dead dacoit half-buried on the bank, and Perreau did not hear the end of the story for a long time.

Another evil-smelling Burmese delicacy is the fruit called *Dorián*. We were told that he who eats it three times will eat no other fruit afterwards. Fortunately I had reached only as far as the third eating when I left Burma.

About May 1886 I was transferred back to the other wing of the 9th Regiment, Madras Infantry, stationed in the Andamans. Perreau was shot by his servant some years later ; but I tried to commemorate him by weaving his appearance and character into those of John Chesham in my novel, *The Spirit of Storm* [111], though of course there was nothing in the life of Perreau to suggest that of Chesham. Kirkpatrick I met again in Edinburgh in 1906.

Before I left Moulmein I began to recognise the futility of my mathematical efforts. I was only a self-taught amateur, whose inventions were sure to be ignored or ridiculed by the professionals ; it was unlikely that my work would ever be published in professional mathematical journals ; and a paper which I had sent to a literary society in Madras did not even receive an acknowledgement (I found in 1887 that it had not been received). The toil was extremely severe and my life was being wasted.

I arrived in the Andamans therefore with a revived ardour for my literary efforts, and a determination to feed my mind on all the wonders of sea and shore in those beautiful islands and to express my experiences in worthy verse and prose. I found for sale an old open ship's boat, with mainsail and jib, and hired for crew two good-conduct convicts, a native of Northern India, and a small man of Mongol type from Further India, both of whom served me well. I also brought my pony Dotty from Moulmein and stabled him on the mainland shore at Aberdeen, opposite Ross Island. After my scanty medical work on the latter was finished early in the morning, I used generally to go either for a long ride on the mainland or for a day's sailing and fishing in the boat. The weather was hot, but there was always a breeze, and I revelled in the beauties of the scenery. We often took food with us, and cared nothing for sun-glare or for the heavy afternoon showers; and all the time I moulded many verses and my romance, *The Child of Ocean*. My age was then 29 years.

The wing of the 9th Madras Infantry on Ross Island was commanded by a Colonel Richmond, popularly known as the Sergeant-Major. The Adjutant, Captain Jackson, was a good fellow; and one of the officers called Tuson, a tall, blond, bland young man with a ruddy countenance, white teeth, and a very critical appreciation of our numerous faults, was the original of Trullo in my *Deformed Transformed*; poor young man, he died literally of home sickness in 1889, and I always blame myself for not having detected his state of health earlier and sent him home. Another officer was Nicholls, a most admirable mimic, who used to imitate a whole regiment on parade, including the colonel, the men, and the band; or a whole church service, including the curate, the congregation, the music, and the sermon preached on the text, "And he went and hanged himself"; and who hated the French and sang French songs in their manner. Among other officers in the station there were Colonel Bromhead, V.C. (of Chard and Bromhead fame in South Africa), and Dr. Keefer, a Canadian. They and another used often to force me (when they could catch me) to make a fourth at whist from after lunch until midnight. I hated the game and the waste of time, and used to play badly on purpose, but had the most extraordinary good luck. I have never been able to endure cards since. I remember, and admired, another settlement officer called Jessop, an extremely athletic man, with the figure of Apollo but a bad temper, tall, and an excellent sportsman. Also a man called Portman,

just of the opposite type, delicate and intellectual, whose house contained a great organ built into the various rooms, who studied the Andamanese and who, in spite of his delicate figure, could outwalk all of us; he had the courage of a lion, and entered Andamanese settlements single-handed (he was nearly killed once later). But I remember best of all, because I admired him most, the Chief Commissioner, Colonel Thomas Cadell, V.C., a most perfect gentleman, who took me for rides and boating trips with him, and reprimanded me for dressing shamefully badly at afternoon tennis with ladies—I had just returned from fishing in my boat! On one occasion I heard him called a liar at his own dinner-table by a half-inebriate junior officer, and Colonel Cadell—well, he took care *not* to hear it, fortunately for the officer. I met him again in 1894 right at the top of the Gemmi Pass, and also in Whitehall—a man whom everyone loved. I have one or two delightful letters from him. He died in Edinburgh in April 1919, aged 84.

When I was first in the Andamans there arrived a shy, delicate, short-sighted young officer with a literary turn called C——, whom for some reason everyone laughed at. But he made friends with me, and I used to take him out to learn to shoot by practising at a mark on a tree, which he could see only with the aid of glasses. One day he said he would like to join the clay-pigeon-shooting competition. He did so and beat the whole of us in a marvellous manner, to the surprise and anger of Jessop and the other crack shots. When I returned to the Andamans in 1886 everyone was talking of a wonderful exhibition which a local amateur mesmerist and thought-reader had given, in which C—— had been the subject or medium. But when he came to me on my return he flung himself down in a chair roaring with laughter and said that he had now revenged himself on the station by fooling them all to the top of their bent. This has always remained in my mind as an example of how easy it is to gull most people by these silly exhibitions. C—— said that even the mesmeriser had been taken in and had begun to believe in his own impostures.

Once a party of us set out in the small Government steamer, the *Nancowry*, for a few days' tour of the islands. There were five Andamanese with us, petted by Mr. Portman, and it was curious to watch their attitude towards the wonders of the steamer, on which they had not been before. They were thunderstruck with the engines—for five minutes, after which they took no further interest in them. I showed them the use

of a burning-glass, which they watched for a few minutes only. Their lack of real interest was like that of a dog or cat; and I have often thought that the measure of a man's intelligence is the interest he takes in the phenomena around him. Those who take none, from the peasant in the fields to the fashionable lady, or the "humanist" who is above the outside world, or the philosopher who strives to weave the mesh of truth out of his own brain, do not always possess too much of it. On the other hand, it was amazing to observe the instincts of these little men of five feet in height. I spent a whole day watching one shooting fish in the shallows with bow and arrows. On another occasion, when we were dressing, after bathing, in a boat floating at about one and a half fathoms, the Andamanese saw a turtle right in the glare of the morning sun, and in a minute one of them jumped overboard and drove the blunt point of the boathook under water into the turtle's shell, bringing it in alive; we white men saw nothing whatever! (But are not these things written of in *The Child of Ocean*?) One day we arrived at a shoal, where we caught on sea lines from the ship an enormous haul of sea-fish and sea-monsters, including a great "gobra," or frog-fish, with a large living octopus in its mouth; and on another day just missed by a few yards an uncharted coral rock, reaching up like a gigantic hand from the green depths of water.

By throwing a baited hook and float into the surf that boiled among the rocks round Ross Island, we caught many curious fish of one or two pounds in weight. Some of these were very good eating, but one, a beautiful azure-coloured fish, with crimson markings, nearly poisoned us all at our mess. One day here my man saw the snout of a huge conger protruding from under a ledge of rock and sniffing at my bait-basket. We gave it a baited hook tied to a long piece of the strongest violin-gut; in a moment, instead of our pulling out the eel, it began to pull us both in, and we were nearly obliged to let go when the gut broke with the strain. On sandy stretches of beach, numbers of small but dainty whiting could be caught by throwing a bent pin baited with hermit-crab into the retreating waves, the fish being jerked out with a bamboo rod upon the sand behind the angler. We caught little from the boat except the usual garfish and kokari, but once brought in *half* of one of the latter, the tail half having been removed by some bigger fish as our catch was being hauled in. One day we were sailing along slowly, with the basket of live-bait hung out astern, when a large hammer-headed shark came up

and followed us, smelling the live-bait basket, into which I was just going to put my hand. There was an old wooden pier at the landing-place of Ross Island, between the planks of which we could watch the fish at high tide. There were four layers of them—minnows on top, six-inchers below these, one or two-footers below these, and monsters at the bottom, each layer feeding on the layer above. Such is the happy life of the denizens of the deep! A soldier caught one of the monsters, a ninety-pound "sea pike." But the prettiest fishing I saw there was done by an old Indian "retired" convict. Kneeling on a catamaran (two or three logs tied together), he paddled it forward to the appropriate spot near the pier and then threw in a handful of small live fish, and the water boiled with the big fish coming up from below. He repeated this twice, but at the third cast, among the handful of live bait, there was one lightly transfixed with a hook attached to a long fine elastic silk line, which the old man kept cunningly coiled in front of him on the catamaran—the art being to throw the hooked fish exactly in the same group as the free ones, all with one hand. Next moment away he would go, paddling hard after his victim; then we could see him playing it some distance away; and lastly, sure enough, he lifted his fish-spear and brought in his glittering prize, generally a kokari, a kind of large, broad mackerel of 10 to 15 lb. in weight, of great vigour and excellent to eat. I think I saw him catch five of these one morning. We generally failed to emulate him, in spite of our fine rods and tackle.

Dangerous cyclones are common in these islands, and about August 1886 the "tail" of one passed over us without doing much damage; and a little later, a day of long rolls of heavy windless leaden clouds, we saw four or five water-spouts around us. One of these burst over the primæval jungle on the mainland, absolutely wrecking the trees over a considerable area. But as a rule the weather was warm, but tempered by breezes; and, when I was not engaged in writing or riding or playing enforced whist, I sailed in my boat between glittering clouds and sparkling sea, or floated over a world of coral on a translucent atmosphere of water, or fished late at night under a million stars, hearing the flappings and blowings of great fish in the phosphorescent sea.

The verses which I wrote here were my "Return to the Muse," "The Star and the Sun," "The World's Inheritors," "Ocean and the Dead," "Ocean and the Rock"—all in my *Philosophies* [114]; and "Calypso to Ulysses," and "Death

and Love " in my *Fables* [113]. But the chief task lay in writing my first " novel," *The Child of Ocean* [109], of which I think I composed the whole of the first draft among the scenes it describes. The idea of writing the book—which should not be what I called a journalistic-novel but an art-novel or epic-drama ; that is, an attempt to figure the scenery of these islands in a sonata of words—had occurred to me when I was here first, two years previously. There was to be, of course, a castaway —not a practical Robinson Crusoe, but a world-embittered philosopher whose soul was to be gradually softened by the music of nature around him. But when I was at Moulmein I happened to read Charles Reade's *Foul Play*, a romance of a good man who had been unjustly convicted of a crime, but who finds himself on an uninhabited island alone with a charming young lady. They fall in love of course ; but presently her father finds her, denounces him as a convict, and carries her away. A powerful and a beautiful episode ; but the book turns into a common London detective story, in which the convict is rehabilitated—to my mind the most horrible incongruity, like a statue of Apollo standing in a slum. I now remoulded the theme in what I thought to be the form of a Greek drama modernised.

Ultimately my castaway became neither a convict nor a philosopher, but a child—though we know of him only when he has grown up. The negrettoes in one of the outlying islands are terrified at the presence of a dangerous " monster," and forsake it. Next we are introduced to the monster himself, hairy, naked, and red, spearing fish in the midst of the surf breaking over the rocks during a storm ; the descriptions of the fish, the minnows dancing on the surface of the foam, and the crabs running over the boulders, being taken from my own experiences. Suddenly, out of the storm there emerges a great ship, which is cast crashing upon the reef and then rolled round into the shallow lagoon. The crew and passengers who have not been washed overboard by the breakers leave the wreck in their boats for fear of the Andamanese, who were then thought to be cannibals. The wondering monster is again left alone. Emerging from his hiding-place, he finds at first only a drowned sheep and a dying sailor ; but presently, awakened at night by the rising moon, he sees something white with a green fire burning on it, lying on the rocks. It is a beautiful girl, still breathing, but insensible, with an emerald on her finger lit by the fairy moonlight. The wild man, like a speechless animal, sits gazing at her.

Next follow the necessary scenes: the terror of Leda Vanburgh when she wakes; she flies the monster and takes refuge on the deserted wreck; he tries to propitiate her with a piece of fish and a dead jungle-pig; her terror changes to pity for the poor castaway; she conquers him, gradually wins back his speech, clothes him, and teaches him; but she herself sickens at the solitude; they live like sister and brother, and she calls him John Vanburgh and busies herself in constructing a raft in order to escape. Gradually, however, she sees with apprehension that his love towards her is changing in character; she falls ill with fever and is consumed with suspicions of him; the Andamanese begin to find them, and the psychic position is illustrated by many scenes, such as that of the Temple of Ocean, the fight with the savages, the procession of the stars, the proof of the wild man's nobility, and his triumph—the old divine drama of pure love. They are about to leave the islands when Leda's friends arrive to rescue her. But they refuse to let her lover accompany her, and the book ends with the old divine drama of death. To this poem (which I should like to have composed in verse) I added the eternal chorus of sun and stars, waves and forests, as I myself heard it on these primæval shores.

Many of the lofty arts of the masterpieces were attempted—the Homeric hush before the storm, the high-piled episodes and the massed music of beautiful words; but these only amazed the honest British reviewer when the book appeared four years later—though most of the "notices" were more than flattering (page 85). Before then I added a Prelude, in order to explain how the Child of Ocean came to obtain his name and to be cast away; but this was written against the grain. Before I left the Andamans I was so engrossed with the pathos of the finally deserted waif, that I wrote some spondaic lines as his last words, called "The Death-song of Savagery" [114]. Indeed the whole romance is a figure of contrast between the present and the primæval.

But these happy times ended suddenly when the 9th Madras Light Infantry was ordered to Madras City early in 1887. I sold my boat, but took my pony Dotty with me. On arrival we went into very disagreeable quarters at Blacktown, close to the slums north of the town. I did little there that I can remember, but I joined the Madras Club, polo club, hunting club, and racing club (I have forgotten their correct titles), and indulged in two more ponies, Eva, an Arab pony, for hunting, and a polo-pony. One day with the

Madras hounds the pads fell to me on Eva, and the brush to a lady.

Shortly after arrival in Madras I was moved to another native regiment there, the 3rd Madras Infantry, which was stationed at Royapetta, in a better part of the town, where we had a more pleasant mess. I should mention here, as a warning to any young man who may possibly read this history, that for months past I had been suffering from depression and an unconquerable drowsiness at about 11 a.m. They were due to the fact that I was in the habit of drinking a bottle of beer every evening for dinner—which I certainly would not have allowed to any patient of mine. I thought, of course, that my constitution could endure anything; but the fact was, that in spite of the constant exercise which I took, the innocent beverage had almost prevented my doing any work, either literary or mathematical, for months. It always affected me *next* day.

My friend Nicholls, 9th Madras Infantry, was an ardent fisherman, and made the acquaintance of Mr. H. S. Thomas, the author of that admirable book, *The Rod in India*, and an Indian civilian. He gave us a delightful dinner during which he described the delights of mahseer fishing (which my father had already told me of). Full of enthusiasm, we bought tackle and started off to fish somewhere (I have forgotten where) in the Cauvery River; but, alas, we caught only one 3-lb. carp, which fell to my rod. A little later I went by myself to the Bhawani River, which flows out of the Nilghiri Hills into the plain south of the hills and of Coonoor and Ootacamund. The railway junction was Metapolliam, and from there I rode twelve or fifteen miles up the river and slept at some mills, where they were making fibre from rushes, though Thomas warned us that the spot was very malarious. The scenery was glorious, but I caught nothing, while my batman secured only one 12-lb. mahseer by going to sleep on the bank with his dead minnow spinning in a rapid. In the last hour of my leave I told him to put his bait into an eddy under a tree; a great fish took his line out about eighty yards; he held on to the reel and was broken! I came here again with my friend Tait in 1891. My third expedition was with a Captain Hamilton (stationed in Madras), to a place some distance south of Madras, where we fished for *Labeo* in a tank within the walls of a great Hindu temple. We started fishing at daybreak, and sat all day on chairs in the shade of a wall on the masonry enclosing the tank, our breakfast and lunch being brought to us there.

The water opposite to us was ground-baited. We had long rods of bamboo with rings and reels; and all day we watched our quill-floats dipping as the clever fish took the fragrant paste off our hooks. Then at the proper moment we struck, and, if we were lucky, away went the fish, from 1 to 2 feet in length, across the tank (some 200 yards square). It was such sport that we could not leave it, and we caught (I think) some hundreds of pounds of these *Labeo*. But I remember especially the scene itself: the red rising sunlight on the great temple; the women coming down the steps to wash, drink, and get water; the big temple drums beating during the religious services; and then the hot evenings and the walk home by twilight. We spent eight days there, and also caught carp and lost a large fresh-water shark.

In the long intervals between riding, fishing, and tennis at the Madras Club, I spent the hot days of 1887 in toiling for the last time, and without success, at the mathematical theory described on page 54. But, to my amazement, I now found that my mind refused to work any more at anything, and that I began to be obsessed with a yearning for the "little distant misty island of the far Atlantic." For change of thought I attempted a course of chemistry at the Government Laboratory at Madras during my leisure—but it was of no use.

I was now in my seventh year of service, but had not even yet received a "pucka" appointment—by which I lost Rs.100 a month; and I was also refused the year's furlough to England, which I was entitled to by the rules of the service after five years in India. The doctors had clamoured for administrative charge of their department, but when they got it did not always prove to be capable administrators; they had overcrowded the junior ranks when I entered in 1881 and then, to cover the error, admitted few candidates for some years afterwards—so that in 1887 there were no junior officers to take my place during my absence. The men of my batch were therefore the sufferers. And, engrossed with my self-imposed labours, I had never even once asked for the two-months' "privilege leave," to which all Indian officers are entitled (if they can be spared) every year, and had never visited the hills for relief from the climate of the plains; so that I was the willing and contented horse which always gets the smallest ration of fodder!

All grits and no coffee, sir,
Make Jones a poor officer.

My health and vigour began to deteriorate rapidly, though

I suffered from no distinct illness (I had never had a day's sick leave, either, except for three days when I caught German measles from a patient in Bangalore in 1884). I lived alone at Royapetta, first in a low bungalow near some rice-fields, and then in a house surrounded by gloomy trees. Here on the eastern coast of India the great south-west monsoon of the mid-year often fails almost entirely; the air is damp but hot; all the morning the sun glares pitilessly; the clouds which form in the afternoon rumble and grumble around, but cast no refreshing rain; all day the kites whine in the brassy skies, the crows come to mock us at the windows, the squirrels shriek at each other in the compound, the mosquitoes devour us; and at night the air is breathless and the darkness a load upon the spirit, in which—

> The eyeless mind
> Gropes like a dumb brain new-born in nothingness,
> Where all the world and all the heav'ns are not.

These were my black days. For six years I had toiled outrageously at almost everything, sparing neither body nor mind; solitary toil which I never mentioned even to my own friends. Now had come the reaction—the punishment; for "man may forgive, but nature never." I could work no more—nor even play; my ponies browsed unsaddled, my books rested unread. Then, moreover, my faith died—the greatest of all faiths, the faith of labour; and I was overcome with the horror of the *cui bono*. What was the use of anything? Years ago I had asked: "Cannot the mind that made the engine make a nobler life than this?" No, it could not. Even if one man could save the world, others would not listen. Even He who died on the cross had died in vain. . . . But are not all these things and thoughts described in every possible detail in my *In Exile* [114], written not then, but some years later from memory? (pages 97, 226).

A bad time which continued till the middle of 1888 and which occasioned nearly all of that poem except the triumphant end of it. I remember the feeling that I was living in a cemetery:

> This is the land of Death;
> The sun his taper is,
> Wherewith he numbereth
> The dead bones that are his.

I remember only too well that booming sea-shore of Madras, where "the sunset dies behind, The full moon springs before"; and those visions of the homeland which I—and many another

exile—saw there (*In Exile*, Part II). Too well also that picture, seen from the same shore, of Death in Part VI :

> Great Death ; not little death
> That nips the flowers unfurl'd
> And stays the infant's breath ;
> But Death that slays the world ;

and the utterance of that melancholy but mocking Moon :

> Fool, all the world is dust ;
> Even I, who shine on thee.
> There perish and add thy dust
> To that sepulchral sea.

Of course this phase is common among hard brain-workers at about the age of thirty—witness John Stuart Mill and the Tennyson of " The Two Voices " and " The Palace of Art " ; and Shelley was coming to it when he was drowned. It is generally due to physical causes, but with me, I am glad to say, was transient, with some relapses. Unfortunately others keep the disease for life and call it philosophy. They are those who persuade themselves that we exist for an object, who fail in finding the object, and then sink into despair. The Epicurean view is the only possible one—that we are born because the great Order of Things has so decreed, and that we continue to exist because the sum of our pleasures exceeds the sum of our sorrows—with me by a large balance.

But I was also vexed by another consideration : I was neglecting my duty in the medical profession. I was doing my current work, it was true ; but what had I attempted towards bettering mankind by trying to discover the causes of those diseases which are, perhaps, mankind's chief enemies ? What was I myself doing to perfect the engine of human life ? I did not decry either literature or mathematics or music for this purpose ; but I was a doctor, and the voice from within answered :

> Abandon Wrath and Ruth.
> Touch not the High, nor ask.
> For God alone the Truth.
> Perform thy daily task.

But I admit that it was a very still small voice that spake even then.

At last, towards the middle of 1888, I could endure India no longer and threatened (demi-officially) to resign if the furlough due to me was not given. It was granted immediately, and I left Madras by ship for England for one year on 28 June.

We touched at Colombo on the way, and some of us (including Dr. Sibthorpe, a man of some capacity, who was afterwards Surgeon-General for Madras on the civil side) took the opportunity of making the beautiful railway journey to Kandy. I wrote two pieces of my *Philosophies* during the voyage, " The Brothers," based on a legend which I found in a nautical book, and " Alastor," which accurately figured the state of mind I had then reached [114].

CHAPTER VI

HOME, BURMA, BANGALORE, 1888–1893

FEW who have not experienced it will understand the intense yearning, not only for home, but for cold weather and cloudy skies, which possesses those who have lived long in the tropics. Although I was not physically ill, I was certainly so " seedy " after my seven years in India that I have now lost memory of most of what happened to me on the way home, until I found myself at Port Said and noticed that the Mediterranean sky was white or even pale blue at the horizon, as compared with the yellow haze of the tropical sky. Then we arrived at Malta —Europe ! And I remember my delight at seeing the live Maltese in the boats and streets, and the women and children with ruddy cheeks. I had arranged to leave the ship here and to travel home by myself through Sicily, Italy, Switzerland, and France—countries which I had never seen. My age was 31.

What a journey ! At Malta I woke up as one from death ; the comparatively cool nights, though it was early July, revived me. The old palaces, the houses, the thronged streets, the fine cathedral (the first Italian one I had seen), the catacombs, and other sights were a revelation. I had arrived at civilisation from savagery—like Aldebran seeing Leda Vanburgh. But I spent only two or three days there, and then took passage on a small steamer for Syracuse. We all dined on deck in the heavenly cool of the evening (July !), and I could speak to no one because everyone was talking Italian, until the Italian captain suddenly said to me, " You may speak English, sir, as everyone here except myself is English."

Next morning we were at Syracuse, the city of Archimedes. I spent several days there in the company of a young Englishman, with whom I visited the Greek and Roman theatres, the quarries, the catacombs, and the poor reedy dull dribble called the Arethusa. We ate Italian food at country inns and drank as much strong Sicilian wine as was good for us—a glorious

time. Next, leaving my companion, I took train for Taormina, seeing Etna and the coast by the way, and stayed there also for several days. I made friends at the hotel with a young Dane, though neither of us could speak a word of the other's language (or any other). We visited the Greek theatre, saw the sights, ate largely at every café, encouraged the beggars, patronised the local musical talent—another glorious time. The place appeared to be owned by an English lady, who, however snubbed us severely when I ventured to address her in English (the hotel waiter said she was an Anglophobe). Vowing eternal friendship with the Dane, I left him for Catania, and thence by ship for Naples.

On board there was a tall, slim young man with long lank fair hair, blue eyes, pale complexion, and an American accent, whom I took to be an American. I made his acquaintance and found that his name was Parlato, his father Italian and his mother English, but that he had been brought up in the States. He was now about to make precisely the same grand tour of Italy as I had prescribed for myself. We decided to go together; which was fortunate for me, as he spoke both Italian and American perfectly, and was a good, well-educated young fellow about to start in the world. West and East had come together. His notions were, of course, democratic, and mine exactly the opposite; but we became the best of friends. At Naples (27 July) we studied the museum, the life, the cafés, cabs, guides, beggars, Pompeii and Herculaneum; but our principal adventure was the ascent of Vesuvius on foot (29 July), and we swore to do it without guides, whom we already hated as hideous pests.

We left the hotel at Naples at daybreak, and railed to Portici (without eating anything). There, of course, the king of the guides detected us sneaking out of the station and whistled up his subordinate villains. They accompanied us, though I swore at them in Hindustani. We followed the guide-book, but lost our way, and Parlato was then so weak as to ask one of our pests to tell us a turning. We found ourselves in a lava-field at the foot of the mountain, and the guide then wanted 20 lire to show us out. Parlato was in despair; but on mounting a hummock I saw the main road 100 yards away! Still, as we walked up the road, relays of rascals walked with us, and we did not get rid of them till Parlato explained that I was a mad Englishman, with an insane hatred of guides, and that he was my keeper! At about 11 a.m. we passed the observatory and the inn and then

came to the cone—already not a little fatigued with an ascent of 3,500 feet without food in the blazing July sun. Then we had to face the steep cone of 1,500 feet of loose scoriæ, which slid down with us as our feet sank in them over the ankles. The funicular railway was not running and the place was deserted, even by the guides. My friend lay down half-way up the cone, but I reached the top of the funicular railway, wandered on a little way among the fumeroles, and then saw that I could never find my way with safety to the crater, and, as I was nearly dead from thirst, had to run down the cone again. My friend meanwhile had gone, but I demanded water at the tiny cabaret at the foot of the cone. The child there said she had no water, but plenty of wine. I drank half a tumblerful, and then set off by a short cut across the lava to the inn, where we had ordered food. By now it was 2 p.m., and I found that the wine taken in my condition had absolutely intoxicated me. I staggered along the barren path, and at last lay down with my head under a small bush, comparing myself to a drunken soldier in India and expecting to have a sunstroke. In about half an hour I woke, and finally staggered, almost as bad as before, into the inn at about 3 p.m. There I found Parlato, having had breakfast, chaffing a circle of guides, whose pride it was never to let a tourist slip through their fingers. I ate an enormous meal, washed down with coffee, and recovered immediately ; and we ran down the road to Portici in the evening sunlight, proud of our victory over the local brigands. All the same, I never saw the crater ; but it was a pleasure to spend an hour on that famous summit, enjoying the glorious view on an almost cloudless day.

At Pompeii the similarity of the ancient Roman houses to the houses of Indians of the present day struck me strongly—the absence of outer windows, the central court or courtyard, the frescoed walls, and the comparative smallness of houses and rooms. I have therefore always distinguished this type of house, the " in-looking " house, from modern glass-windowed " out-looking " houses—a distinction of sanitary importance.

From Naples we travelled to Rome (30 July), Florence (4 August), and Venice (7 August), seeing the wonderful sights—to an exile from India a vision of beauty. From Venice, after a few days in Milan (10 August) with Leonardo and the Cathedral, we went through the St. Gothard to Lucerne (11 August). For the first time since my childhood I saw great snowy mountains ; and now, absolutely

revived by the cool, I felt all my old spirit bounding within me. One day we walked up the Rigi; and it was then that I thought of a story which should combine and contrast within itself all the main great types of human character by means of a construction which Byron had commenced but never finished in his *Deformed Transformed*, to which I now proposed to add a divine figure in the form of the dwarf's twin sister, thus giving the tale the symmetry required (*see* pages 90 and 104). I proposed to lay the scene in these mountains, but wrote nothing for a year. After Lucerne we travelled to Paris (13 August), where I said farewell to my companion, and then proceeded to England. I had some letters from Parlato afterwards, but never saw him again. I even wrote recently to the British Ambassador in the United States and to Malta to make inquiries about him, but could find no trace of him. (Since this was written he has been found—Dr. Parlato-Hopkins, of La Tour-Vevey, in Switzerland. He gave me the dates quoted above.)

The sky clouded over when the train left Paris, and I arrived at Newhaven in a drizzle. What delight! Then I went to the Isle of Wight, where I found my parents, brothers, and sisters living in Lothian House, facing the Solent and Portsmouth, on the eastern outskirts of Ryde. That was some time in August 1888.

My brother Claye, of the 14th Sikhs, was also at home on furlough. Though I had been a quarter of an inch shorter than he when I left England in 1881, I was now a quarter of an inch taller, so that, unless he had shrunk in the interval, I must have grown half an inch between the ages of 24 and 31. My brother Charles (now a Major-General), had entered the Army, and my brother John was studying for it. My father still painted pictures in the mornings and then walked out with us along the shore; but, like many other retired officers, he evidently fretted for want of full occupation. On the other hand, my good mother had quite enough to do in housekeeping for her large family.

I determined now to interest myself much more in my profession, especially in sanitary work, the importance of which for India I fully recognised. Also at that time bacteriology, created by Pasteur and Koch, was coming to the fore, even in England; so that I determined to take the new Diploma in Public Health which had just been started in London by the Royal Colleges of Physicians and Surgeons combined, and then to undergo a course of bacteriology under Professor Klein in

London. For the former I went to study with Professor W. R. Smith for some months, and presented myself for examination in December 1888. A question regarding population was set in one paper, which I easily solved by a method of my own requiring expansion by means of the Binomial Theorem. When I came up later for *viva voce* examination, the examiner smiled and said that I was evidently an expert mathematician, for, though he did not understand my method of solution in the least, the result was correct to the last place of decimals. Professor Smith told me afterwards that it was this answer which sufficed to pass me, since the rest of my work had been poor. I believe that this was the only occasion when my mathematics has been of the slightest service to me!

The reason why my work was poor was that I had become engaged in December to Miss Rosa Bloxam. We were married at All Saints, Norfolk Square, by the Rev. D. M. Gardner and the Rev. Aston-Binns on 25 April 1889. My father and mother, brother Charles, and my three sisters were present, two of them being bridesmaids; and my old friend, Frank Aston-Binns, who was now the very successful Master of Modern Languages at Rugby School, was my "best man."

For our wedding tour we went to Edinburgh, Loch Awe, Oban, and back by the Trossachs and Glasgow. The weather was beautiful and even very hot, and I fished for trout at Loch Awe, Oban, and the Trossachs. In the latter I was turned off the water one Sunday by three angry (but I thought hypocritical) Scotsmen. At Oban we visited a Gaelic poet, who was postmaster at Loch Nell, and he recited his verses to us—we remember only the gutturals. At Edinburgh we visited the Forth Bridge in the making.

On return from Scotland we lived near the Aston-Binns in rooms at Reigate, Surrey, and my father and mother stayed with us. The summer was a glorious one, and we sometimes sat all day long on the edge of the downs, among the lush grass and the fox-gloves, overlooking the beautiful plain below—what a change from India! But the Indian authorities had kindly given me two months' extension of leave to study bacteriology under Professor Klein, F.R.S., in London, and I was obliged to sacrifice those lovely days to this duty. I learnt much of the subject; of the microscope, of incubators and culture media, all of which was to be of value later, and am greatly indebted to that capable teacher. I *believe* that I was the first member of the Indian Medical Service either to

obtain a Diploma of Public Health or to study bacteriologythus, but am not sure. This was in 1889.

Now about my romance, *The Child of Ocean* (page 72), and the comedy of its publication [109]. On my homeward voyage I had described the plot of it to one of the ship's officers in order to obtain some information about the law of storms. He advised an introduction to explain how the boy came to be cast away in the Andamans ; and I therefore drafted a somewhat turgid Prelude, according to which the castaway was the son of a South Sea adventurer. He was born and brought up at sea, but when the crew was massacred by Malay pirates he was allowed to live and was sent adrift in the ship alone with the dead. There was some grandiose invention here, but the effect was inartistic, and the Prelude was adopted only tentatively. Next, the whole book was fair-copied by about March 1889, and went the usual round of the publishers, begging for birth. First Messrs. ——— rejected it, saying that they liked the Prelude but found the book itself lacking in " likelihood and human interest." I then took it to Messrs. ———, who accepted it on a glowing report from Mrs. Lovett Cameron, the popular novelist, who described it as " most original and exceedingly powerfully written," and whose opinion was just the opposite of that of the previously mentioned publishers. Messrs. ———'s printed form of agreement was so vague, however, that after paying £75, half the cost of publication, I should still be liable for many charges ; but when I put this little difficulty before them they found another " reader," who spelt *loath* for *loth*, and who advised that " the horrors described are most revolting " ; and they then wrote to me that " no art could reconcile the ordinary English public to the idea of a pure, virtuous girl falling finally in love with a wild monster such as you have in the beginning described " ; and they thought that this was opposed to the standard of good taste accepted in this country, and withdrew from the bargain. The book was then refused by Messrs. ——— ; but was finally taken in July 1889 by Messrs. ——— on the following conditions : (1) They were to bring out an edition of 1,000 copies at six shillings each and to pay me a royalty of one shilling on each copy sold, and a royalty of one-sixth the published price on cheaper editions. (2) They were to hold the copyright for three years. (3) They were to have first refusal of any subsequent works of mine. (4) " If the book was to turn out a failure " I was to pay the cost of production, not exceeding fifty pounds, by 31 March

1890. The word *failure* was not defined; but as I was on the point of departure to India for another five years I was obliged to accept the risk, and signed the agreement on 31 July, the day before I sailed. The whole edition was ultimately sold out. Not only did I not receive a farthing of royalty, but I had to pay for advertising; and the publishers (under a changed name) pressed me, my relations, and even the Horse-Guards and the India Office, for the full £50 payable for "failure"!—which, of course, I did not pay. Shortly afterwards the original firm which made the agreement with me seems to have disappeared or changed, and the book cannot now be obtained, even second-hand. I still have all the documents.

I was obliged to take the manuscript with me as far as Egypt, in order to make final corrections, and never saw any proofs, so that the book is full of printers' errors. Worse still, the publishers thought fit to bring it out as a boys' book, with a horrible picture on the cover showing a pirate (whose nose is where his ear ought to be) shooting at a schoolboy out of perspective on the bowsprit of some antediluvian craft. It appeared early in November 1889 I think, and quickly received fifteen press notices, most of them very flattering indeed in spite of the cover. But my elation was dashed by the fact that the reviewers contradicted each other flatly regarding almost every detail in the book. Thus *The Court Circular* found it to be nicely bound and suitable both for boys and for girls, while *The Standard* castigated the publishers for bringing it out as a boys' book and added that "a book more unsuited for boys and girls could hardly have been written," and *The Literary World* found it to be "a clever and remarkable book which need not be put within the reach of young people." *The Morning Post* and *The Athenæum* disliked the Prelude, but commended the story itself, but *Woman* commended the former and did not mention the latter. It is ungrammatical according to *The Sunday Times*, but Mrs. Cameron said the grammar was good. I was compared to Victor Hugo, Rider Haggard, Stevenson, Clark Russell, Charles Reade, and "Seafarer" (what cleverness in me to write like all these famous people simultaneously!). *The Standard* said that the story "is drawn with a master hand and is a work of real genius"; *England*, that "the wealth of imagery throughout this volume is a continued wonder"; and *The Liverpool Courier* that "it will find its level among the best romances that have been written"; but *The Spectator* does not commend it to its readers, "though there is undoubted cleverness in it. This we hope to see

better applied on some future occasion." *The World* condemns " its extraordinary inequality," but *The Sunday Times* finds that " it is handled with a mingled power and discretion which magnify alike the author's imagination and good taste." *England* " concludes that I do not exist at all, but that ' Ronald Ross ' will be heard of again ! " Yet reviewers complain that authors do not always appear to be so grateful to them as they should be ! I was, however, truly grateful for a spontaneous letter dated 10 November from Mr. Rider Haggard, whom I did not know then, but who praised the book. *Lætus sum laudari a laudato viro ! . . .* I have tried several times to get the book republished, but no one will take it.

Laden with a full box of type-cultures of bacteria from Dr. Klein's laboratory, we embarked on 1 August 1889, in tropical weather, at London, on the British India steamer *Navarino*, which we nicknamed the Never-in-oh, because of her leisurely progress ; and we had a very hot pasasge down the Red Sea.

On the way out to India we stopped at Colombo and visited Kandy, with its beautiful botanical gardens and its Buddhist temple built round " Adam's footprint," and then spent a day in the fine harbour of Trincomalee. We arrived at Madras on 6 September 1889, when I immediately received orders to go on field service in Burma in medical charge of the 2nd Regiment Madras Infantry, with the Chin-Lushai Expedition. I was obliged to leave my wife (who had never been away from home before) in an hotel at Madras by herself, and started on 21 September in a transport. Landing at Rangoon, we then travelled by train and river steamer to Pakoko, the base of operations, a village on the right bank of the Irawadi, a little below Mandalay. Owing to the kindness of Surgeon-General Roe, of Madras, my wife was soon allowed to follow me. She left on 2 October, and arrived in Pakoko on 16 October, where the police were so very kind as to lend us their bungalow. I remained there till early in January (? 3rd) 1890 when I was ordered up to the advanced base at Gungaw, some eight days' march, and Kan, two marches farther on. I reached Kan on 20 January, riding on Dotty ; but on 27 January was ordered to bring back a convoy of sick via Kalewa and Myinjyan down the Chindwin River to Mandalay, and then back by Pakoko to Gungaw. I must have arrived at Pakoko on about 8 February, but could stay there only two or three days, after which I returned via

Pauk and Tilin as before to Gungaw, which I reached on or before 24 February. I stayed there till about 14 April, when I received orders to return to Madras. I rejoined my wife at Pakoko, and we left Rangoon for India on 29 April 1890.

On our arrival in Burma the country was green after the rains, and Pakoko was a land of hedgerows and wild-flowers; before we left it was a desert, with dusty paths through brambles! I shall never forget the beauties of the marches from Pakoko to Gungaw, which I trod three times, and described in letters to my wife. The stages were, I think, Kandla, Libia, Pinchong, Pauk, Chyoungo, Yebu, Yedu, Ayaban, Tilin, Swekendain, Tawma, Mingwa, Gungaw, Minza, Kan. The delights of such marches in the East at this season have to be experienced to be understood. We generally started at about 4 a.m., and all of us walked or rode almost in our sleep, in the cool darkness before dawn; scarcely anyone spoke, whistled, or sang, and I rode Dotty or plodded along on foot. Then, in an hour or so, there was the zodiacal light above the trees; then a bird called; then a man whistled; then came the white light of dawn, and we all woke up and laughed and talked, the horses neighed, the doves cooed, the birds sang, the dogs barked. At 8 or 9 a.m. we came to the camp, where we generally remained all day, though we sometimes did a short evening march. At first out of Pakoko the land was flat, sandy, or dusty; but at Pauk we saw mountains before us, and between Yebu and Yedu we ascended them, and I wrote the following to my wife:

"We have had the most beautiful scenery the last two marches, and this morning at daybreak I almost saw Pakoko. You must know that we are crossing a range of hills about 3,000 feet high . . . passing up and round a succession of great gulleys full of immense jungle . . . every now and then there is a pretty wooden bridge. . . . You can imagine how fine this looks by the early morning moonlight, and then by the cold daybreak, and lastly by the first red sunlight. But you cannot imagine the stillness of everything before dawn and then the awakening of the birds. Doves begin to coo in every direction and jungle-cocks to crow. Well, when we reached the highest part of the ridge there was a break in the trees and we saw a vast plain with Popa Mountain in the distance and a faint silver streak for the Irawadi. I could make out almost exactly the position of Pakoko. . . . You

little thought that I was almost looking at you nearly a week after I left you. . . . You see I am only 50 or 60 miles away from you now."

The climate was simply delicious, and at Ayaban and Tilin I fished in the beautiful streams, surrounded by jungle and mountains, after the morning's march. On the way back with a convoy of sick we travelled in Burmese boats down a river to the Chindwin, and I wrote in the boat on 2 February to my wife :

" It is now the early morning and the sun is just breaking through the mists which hang about the immense trees of the jungle. The river is roaring along, carrying us through small rocky hills. It is splendid : the sun touches one or two trees which stand out from the hollows full of mist. I showed it to Wood [a sick officer], who said, ' Why, it's all misty, man,' as if the mist spoilt the scenery ! . . . I am now sitting on the very stern of the canoe and see the water sliding away under me. The whole river is full of huge logs and dead trees— it is curious to see how the river eats into the forests. . . . We hear all sorts of strange cries of wild birds in the jungle."

On my return march to Gungaw in February the trees were covered with crimson blossoms, the spectacle at Tilin being lovely. Close here, at Swekendain, I wrote the verses to my wife, called " Message " in my *Lyra Modulata*, on 20 February, in blank pentameter, with an echo-rhyme at the end of each stanza, the best description which I could give of the scenery. When I returned to Pakoko in April the woods were almost bare with summer heat. My man, Kurrim Bux, and I marched double marches (averaging twenty-five miles a day) and lived on rice, dhal, onions, and a few birds shot on the way. On the day I wrote the " Message " I told my wife :

" I feel that I could walk all day and every day with only one meal a day, as I am in splendid health ; . . . a very little suffices me now. I have been three days eating one fowl. I went 20 miles to-day and felt at the end as if I had just got out of bed, almost."

It was not so pleasant for my wife at Pakoko, which was hot and disagreeable. The Commissioner and his wife, Mr. and

Mrs. Fleming, and Mr. Bestall, a missionary, were very kind to her; but the time was an anxious one for us, as we did not know what the Government was going to do with me, and I now had nine years' (!) service without a pucka appointment, by which circumstance I had lost something approaching £1,000. Our pleasure was greater, however, when we were told in April that I had been finally appointed Staff-Surgeon at Bangalore (my old temporary post under a new name), doubtless owing to Surgeon-General Roe's kind interest.

There were plenty of humours on these marches. Before starting on the first one I asked Evans, a burly, witty, and capable veterinary surgeon, who was temporarily in charge of the transport (the main body of troops had left weeks previously), how much baggage I was entitled to. "There is all my kit," he answered, pointing to a box about the size of a biscuit-tin. "What are all those big packages ranged round your room, then?" I asked. "Oh, those are commissariat details," he replied. Strangely enough at our first, and every subsequent, camp we sat down to sumptuous repasts, with real tables and chairs, all of which had, of course, come out of Evans's little kit-case! We often slept in punghi-kyowns (monasteries), now known to be favourite haunts of malaria-infected mosquitoes, but escaped because it was the cold weather; the officers in advance of us had suffered badly.[1] During my stay of several months at Gungaw I fished in the great pool of the river below our mess-house, and caught nothing, in spite of all attempts with fly, minnow, worm, or bait. I began to conclude that there were no fish in the river, when a forest officer threw in a dynamite cartridge. It was a sight to see the monstrous stunned fish that were hauled out by his Burmese followers—some as big as the little Burmese themselves. Afterwards I saw the natives catching these fish with night-lines baited with lumps of a peculiar kind of clay. Of course I could do no bacteriological work during these times, but I began to study mosquitoes seriously; bred them from the larva, removed them from our quarters (as in Bangalore, page 58), and distinguished two main varieties, which I called grey and brindled mosquitoes—that is, the genera *Culex* and *Stegomyia* respectively (page 219). I could get no books on the subject in India.

But my inner self was engrossed all the time by a new and

[1] Some account of this sickness is given in my Parke's Memorial Prize Essay on Malaria [16].

bold scheme which was conceived in the Alps (page 82). This was no less a one than a drama which should bring most of the main types of human character, physical, mental, and moral, upon the stage all together, to be figured and contrasted within one great symmetrical design. Like other neglected writers, such as the Elizabethans, I did not scruple to seize for my purpose the incomplete designs of others, and I took Byron's fragment, *The Deformed Transformed*, and finished it. Let me describe the effort briefly, lest the reader be already weary of these literary excursions.

In Byron's virile poem a dwarf is reproached by his mother for his deformity ; but presently the Evil One appears to him and offers to change him into the most perfect form of man (Achilles) ; he accepts, but when the metempsychosis is complete, the Evil One takes for himself the discarded body of the dwarf, whom he elects to follow as Mephistopheles follows Faust. This is a great invention and a great parable of body and mind—more dramatic because more elemental than the variants of Marlowe and of Goethe ; but Byron now wanders off into unselected episode and ends because, I gather, he had not found the true climax. I think it was in 1888 that I heard Rubinstein's beautiful opera based upon Lermontoff's Demon, in which the Spirit of Evil seeks human sympathy and love and fails. I saw that the two stories (both derived from anterior sources) should be combined, and therefore created the following : The time is 1495. An Italian exile, Morva Neroni, has taken refuge in the heart of Switzerland, where she has brought up her twin children, Astrella and Zozimo. They are poor, so that Zozimo is obliged to enter the Count Reichenfels' inn in the valley as tapster ; but, being feeble, dwarfed, and deformed, he is ridiculed and tormented by the Count's servants and jester, the hideous Gangogo, more deformed than himself, and is reviled, even by his own mother. But he is pitied by the Count's daughter, Lelita, and is presently rescued by his sister, Astrella, the chamois-huntress and Artemis of the Alps, a human-divine figure. He falls in a fit, and, being presently left alone in the great gloomy room of the inn, sees a magnificent stranger seated at the table ; and the metempsychosis follows. Some months later the Count gives a festival at which he promises his daughter to the handsomest man living, and Zozimo, in his new form, wins her. But he is always followed and tormented by the figure of his former self. Then falls the tragedy. The Evil Spirit is rejected by Astrella, and at the moment of his triumph Zozimo is forced back into his old

body. Astrella flies with him to the icy peaks of the Galenstock, where she is flashed dead by her terrible lover in the arms of her poor deformed and dying brother.

On these long marches in Burma, among those sunlit valleys and those million-blossomed forests, I thought out that tale—which is the tale of humanity, of all of us. With what enthusiasm it filled me! What wild invention—plot, counter-plot, and shadow-plot, episode heaped on episode, character upon character, the lispings of the lovers in the garden, and the last despair of the dreadful Spirit upon the peak! It was the delight of creation; but to me in those days melancholy was music, sorrow a joy, and tragedy sublime.

We were obliged to wait for several weeks in Madras after our arrival, and a sad event occurred during that time. The shipping authorities had refused to allow us to take my pony Dotty on our ship, so that, at the last moment, I was obliged to leave him behind with agents for subsequent shipment. But these people omitted to provide him with a mat to stand on upon the slippery deck. A gale came on and he fell, and then broke loose, scampered about the deck, and cut himself all over against deck machinery. He was mere skin and bone when he arrived in Madras, was only just able to whinny when he saw me, and then died of the inevitable tetanus (lockjaw). I refused to have him shot—a barbarous procedure. How many men would like to be "put out of their pain" in that way? I had owned him for five years.

On arrival at Bangalore on 20 May 1890, I took up the Staff Surgeoncy—formerly the Garrison Surgeoncy—as ordered—my first pucka appointment after nine years' service. But, though highly coveted, it was only a three-years' appointment, not capable of renewal. We lived on the High Ground, first at St. Helens, and then in a delightful house facing the golf ground, called Uplands. I played much golf and was secretary of the Golf Club for a time.

The place and the work were described in Chapter IV. I was medical officer to the General and his staff, to other officers, to the families of officers who came here to avoid the heat in the plains below, and to various detachments of native troops; and had a small hospital and was allowed private practice. There was little surgery to be done, and my surgery became rather rusty; but my medical practice was considerable, especially among officers' families. Except for some of the British regiments, there was no good water supply, and the conservancy was certainly not perfect, with the result

that there was a considerable amount of enteric fever, of other and milder fevers, and of bowel complaints and parasites, especially among the white children. But there was little or no malaria, except among people who had been infected elsewhere. There was enough sickness to keep me often in a state of anxiety, especially with regard to the little ones who were so unfortunate as to possess what I called " wild mothers "—poor distracted ladies who knew more than the doctor ; who swaddled their children in woollens during the hottest weather " in order to prevent chills " ; who fed them on concentrated meat-essences " in order to keep up their strength," and who gave them medicines which they " had found at the chemist's " ; but who, when the inevitable shrieking began sent for the doctor at any hour of day or night. There was no great Dr. Truby King in those days.

But I had now determined to labour really at my profession, and therefore read the medical books and journals within reach ; but they did not help me much. The fevers, especially the slighter ones, were the chief problem. They were then thought to be of " malarial origin," but were really typhoid, paratyphoid, intestinal affections, and sometimes fevers of the dengue group. Hence I began to think that even malaria itself may be intestinal, studied the subject laboriously from this point of view, called the fever enteroseptic fever, and wrote four papers on it [11]. I did not know that the French writer Broussais had anticipated me in this conjecture, and that, as regards malaria at least, we were both wrong (but the name enteroseptic fever was much adopted in America subsequently). At that time Laveran's discovery (made in 1880), that malarial fever is caused by a parasite in the blood, began to be talked about in India ; but unfortunately those who thought they had found the germ had (with the exception of Vandyke Carter) merely mistaken natural objects in the blood for it. I set to work with energy, but detected their error at once, and hence became sceptical regarding the whole theory. At the same time I became equally (and rightly) sceptical regarding the theory that the so-called malarial fevers are due to aerial miasmas arising from marshes, because, if this were the case, almost everyone living in the vicinity of marshes should become infected. I worked hard at the microscope and wrote four more papers exposing these errors [13] (*see* Chapter VIII).

Some months after arrival at Bangalore I completed my drama, calling it *The Deformed Transformed* (in a double sense), in honour of Byron, and then had fifty copies printed

at the *Bangalore Spectator* Press, with a preface dated 3 November 1890. Copies were sent to my wife's brother and sister-in-law, Mr. and Mrs. Cazalet Bloxam, who took great trouble over finding a publisher. By my agreement with Messrs. —— regarding *The Child of Ocean*, they had first refusal of subsequent works of mine, and they now held up the drama for a long time. At last Messrs. Chapman & Hall published the work at my expense—nearly £60 for 300 copies. This edition was issued in 1892, and contained a scene (Act IV, scene iii) not in the first edition, and some other emendations. A few reviews appeared, fairly favourable, but I doubt whether any copies were ever sold. I scarcely expected a better fate. Beside my father, to whom I dedicated it, I doubt whether a dozen people have ever read the work. But a few years later I told men how to save their own lives by the million, and they paid no attention whatever!

We spent three years at Bangalore. My daughter Dorothy was born there on 15 November 1891, and my daughter Sylvia on 20 January, 1903. We had a pleasant house, played golf, went to the club, and saw a good deal of society.

Our *mali* (gardener) was a dear old Indian, whom I tried to commemorate in my *In Exile*. He was so fond of flowers that one day he stole a whole row of a new kind from my next-door neighbour, Surgeon-General Inkson, an iracund gentleman; and he was locked up in the police court for three days in consequence; and I suppose that only my own indifference to horticulture saved me from sharing his fate, as he had planted his acquisition in my beds! I remember him peering out pitifully from the grated window of his dungeon, from which I rescued him by paying his fine.

Our leader at golf was Mr. Tait, of the Mysore Education Department, the son of Professor P. G. Tait, of Edinburgh (the mathematician and interpreter of Hamilton's quaternions), and brother of the well-known golfer, who was killed in the South African War and who drove a golf-ball many yards farther than a golf-ball was mathematically capable of being driven—according to his father. My friend was said to be a better golfer even than his brother, and held the record for the Bangalore links of twelve holes with a score of forty-six only. He was a wonderful man with the cleek, and was kind enough to go round often with me.

In 1901 we went together to fish in the Bhawani River, near Metapolliam, at the foot of the Nilghiri Hills (page 74). We wasted two days of our leave by losing the way to our

rest-house by the river, so that we had to walk all the way back at night to Metapolliam, which we would never have reached if we had not come at midnight to a native village where we obtained guides, with much difficulty. We hired two round basket-coracles, in the centre of which we sat on a box or other raised seat, while the boatman kneeled in front of us and paddled us forward towards our quarry. Starting at dawn, we walked some miles up-stream, the boatmen carrying the coracles, and then fished all the way down-stream through pools and rapids back to our rest-house, some miles up the river from Metapolliam. We caught no mahseer, but had good sport with Carnatic Carp by throwing artificial flies or grasshoppers under large trees in which monkeys were disporting themselves—these fish being about 10 lb. in size, but sluggish fighters. At the end of our leave we started at daybreak to walk up the road towards Coonoor, proposing to have a picnic-breakfast some thousand feet up the hill. But when we had breakfasted we felt so fit that we continued our climb and finally reached Coonoor, 5,000 feet above and twenty miles from Metapolliam. Here we ate a huge luncheon, and then ran down the road by highway and short-cut till we reached the plain ; after which we had to trudge for five miles along a dusty flat road back to Metapolliam, which we reached about 7 p.m. I wore only a thin pair of shoes or boots, and my feet were so blistered that I could not walk for three days afterwards. Tait was a great walker, and told me that he walked seventy miles at a stretch some years later in the neighbourhood of Bangalore. On this occasion he contracted malaria at the rest house near Metapolliam. I did not, because I slept in a mosquito net ; and I remember noting the difference at the time, though no one then suspected mosquitoes of carrying malaria. In the heat of the day both of us used to lie down with our clothes on in the rapids to get cool, and kept on the wet clothes till we reached home !

My acquaintance with Tait encouraged me to seek help from his distinguished father, Professor P. G. Tait, regarding my method of geometry, which I had invented here in Bangalore in 1883 (page 58). Consequently, some time in 1891, I wrote to him, with an introduction from his son and an article describing my thesis, asking him whether the work was original and worth continuing. Professor Tait's reply, dated 27 January 1892, was most discouraging. He said that he himself had abandoned the study of quaternions recently, and that he had therefore sent my thesis to a friend (whose

name he did not give), who evidently thought little of it. I therefore put away my papers with a sigh ; but after I returned to England in 1899 found the whole of my method set forth in a great recent textbook. In fact, it had been discovered long previously by Grassmann, though neither Professor Tait nor his expert friend seemed to know this. Also both Grassmann and myself were wrong, and I published what I believe to be the true method in 1901 [101] (page 415).

At that time I again spent much of my leisure over Greek and Latin poets—toiling hard with grammar, dictionary, and translations ; but remained a dull scholar. The prosody interested me much, and I could not help thinking that the laws of " long " and " short " were really laws rather of euphony than of measure, just as similar laws are required for English when we attempt measures outside the regular ones now in common use. In my " Torrent," " Ode to Night," " The Star " [115], and other pieces I tried several new rhythmic schemes, which certainly demand something very like long or short syllables if any euphony is to be attained. Apart from this, the studied perfection of the classical verse most attracted me, though modern criticism tries to discredit perfection in verse altogether and to substitute spontaneity (? gush) for it. I concluded that if English verse is to emulate the classical verse it must be written in phonetic spelling (page 508).

Among our many friends were Surgeon-Major and Mrs. Dobson. He was Residency Surgeon, and his sister, Miss Fanny Dobson, stayed with us for a long time ; we held the record for mixed foursomes at golf. Dr. McGann and Dr. Benson, of the Mysore medical service, were great golfers, and with their ladies, and Mr. and Mrs. Wolsey Smith and Mr. and Mrs. Rice, of the Archæological Department, formed a very pleasant and cheerful society on the High Ground, which was some distance from the military cantonments of the station. There were also the Resident and the General and various members of their staffs, and much tennis and badminton in the Cubbon Park ; and a friend (a Mr. Hudson, I think) and I went fishing in the hot weather of 1892 near the Mysore " keddas," where the wild elephants are trapped by means of a large system of dykes cut through the vast jungles.

But in spite of these pleasures I now began to feel somewhat depressed again. The news of my father's death on 20 June 1892 reached us by telegram. My books and Professor Tait's letter had been great disappointments to me, and I had been obliged to go all the way to Murree in the

Himalayas and back in connection with a melancholy family trouble. In spite of its beautiful climate, Bangalore was not a very healthy place and did not suit me; my studies on malaria did not advance, and I began to be oppressed again with some of the moods of 1888. The fact is that the Indian climate cannot be endured so well by those who have been born in it. The responsibilities of my professional work were also often very trying, chiefly because we really did not know at that time the nature of most of the illnesses which we were obliged to deal with.

I wrote a good deal during the intervals of other work—and play—because I was then thinking of taking to literature as a profession after my first pension became due in 1897. Numerous clever stories called "turn-overs," written by various officers in India, were appearing in the Anglo-Indian Press—of which Rudyard Kipling's first efforts are now known to everyone (I did not read one till 1898); and I commenced a light romance on my Burmese experiences, which would probably have been a success if I had ever finished it. But I did not care for this popular class of writing, which I call journalistic or arithmetical art, because it deals with trivial particular instances. For me real art must always be algebraic—the general equation perfectly set in symbols and covering any number of particular instances. I wrote two more fables, "The Toad and the Fays" and "The Piteous Ewe" [113]. In the latter Dame Ewe complains to King Lion that Lord Wolf has eaten her children. Much incensed, the King calls a full court and hears the case argued. His Prime Minister, the Bear, lays the fault on the previous Government, and the leader of the Opposition, the Tiger, rebuts his calumnies with anger. The King asks the Lord Chief Justice, the Elephant, to decide—who concludes that the Ewe has produced no evidence and is probably libelling the Wolf; at which his infuriated Majesty roars for dinner and the court dissolves in a hurry:

> But in the scramble no one knew
> (So says the Saga that is true)
> What happen'd to the Piteous Ewe.

I also wrote four more dramettas (page 36), "Otho," "The Triumph," "Evil," and "The Marsh," all of which were published in *Psychologies* [121] about twenty-seven years later. "Thought and Action" [114] is, however, a demand of the conscience for more active work; and in "Vision," a discussion between Learning and Science [114], the call of Science becomes

still more insistent. This is seen still more in "The Star," in "Indian Fevers" (of which I dare not publish one of the stanzas), and in "The Indian Mother."

THE INDIAN MOTHER

Full fed with thoughts and knowledges sublime,
And thundering oracles of the gods, that make
Man's mind the flower of action and of time,
I was one day where beggars come to take
Doles ere they die. An Indian mother there,
Young but so wretched that her staring eyes
Shone like the winter wolf's with ravening glare
Of hunger, struck me. For to much surprise
A three-year child well nourish'd at her breast
Wither'd with famine, still she fed and press'd—
For she was dying. "I am too poor," she said,
"To feed him otherwise"; and with a kiss
Fell back and died. And the soul answerèd,
"In spite of all the gods and prophets—this!"

This was at the Bowring Civil Hospital at Bangalore, of which I was in charge during Surgeon-Major Dobson's absence; and it contains the whole subject of science in government as compared with the faked politics of some people. I add:

INDIAN FEVERS

In this, O Nature, yield I pray to me.
I pace and pace, and think and think, and take
The fever'd hands, and note down all I see,
That some dim distant light may haply break.

. . . .

The painful faces ask, can we not cure?
We answer, No, not yet; we seek the laws.
O God, reveal thro' all this thing obscure
The unseen, small, but million-murdering cause.

But as early as 1890 I had formed a new project—that of describing all my Indian thoughts and experiences, grave or gay, pleasant or unpleasant, in a suite of sonnets which was to be called *In Exile*. They were to be introduced by the sonnet "High Muse," in *Philosophies;* but I soon concluded that this form, short as it is, was too bulky for many of my themes, and I tried others. All five-foot lines were too capable of redundancy, and even the four-foot line of "In Memoriam," and other measures invented by various writers and myself, would not quite fit my ideas. At last, on my visit to the Bhawani River in 1901, I settled upon the three-foot line in three quatrains, which was finally adopted (really four crochet-feet to the bar-line). This scheme contains only thirty-six feet in each "sonnetelle," and therefore seems to be capable of the utmost fire of concentration possible—and in my opinion

concentration is perhaps the greatest virtue in verse. The passionate stanzas could be moulded like hot metal, while by suitable elocution the three-foot lines could be run together into the long roll of the Alexandrine (as was done when they were read publicly in 1917 (page 517). The partition of each sonnetelle into three stanzas allows of a beginning, a middle, and an end—agreeable to my love of form. The chief demand of artistry, variation within uniformity, could be met by giving each sonnetelle a different sentence-construction within its narrow limits; and I enjoyed the effort as a musician enjoys writing fugues, a sonneteer sonnets, or any acute mind epigrams. Each piece was meant, as it were, to be inscribed on a separate piece of stone. The whole was designed to contain descriptions of scenes and people, thoughts on science, art, and affairs, satires, and pæans, and to end in an integration of opinion which was to be the final philosophy! Alas, the metal does not always fill the mould.

The poem as a whole is, therefore, not an artificial "literary" product, but a natural growth, a kind of psychological diary in verse—continued from 1901 to 1907, except when I was at home in 1894–5. Unfortunately the pieces were mostly drafted on separate slips of paper, to be copied, amended, arranged, and rearranged in several note-books, so that the original order of them is lost. Certainly they were commenced at Bangalore, but in my Madras moods of 1887–8 (page 76), too often the moods of the exile. Many stanzas were added subsequently, and the work ended triumphantly with the mosquito-discovery at Secunderabad in 1897 (Chapter XIII), but was not finally arranged and printed till 1906 [112], and was not published till 1910 [114]. Owing to many omissions, it ultimately took quite a different character to that originally intended (pages 493 and 506).

The Death stanzas of *In Exile* (Part VI) were written, I think, just before we left Bangalore in 1893. I was again not at all well then. I had tasted of great sciences and arts, and had myself attempted something in some of them, but everything I had tried had failed. The failure did not exactly depress me, but it drove me apart and aloft into peaks of solitude. My experiences were solitary ones; my friends were only the friends of my outer self; my inner self lived upon the snows of an inaccessible summit. Such a spirit was a selfish spirit, but nevertheless a high one. It desired nothing, it sought no praise, it achieved nothing; was not really stale, weary, or sad; had no friends, no fears, no loves, no hates.

It lived in mist and waited for something—as told in some verses of mine called "The Voice," which also I dare not publish. At the same time it condemned me to some acute self-criticism, which is shown in the following stanzas written early in 1893 at Uplands during the hot weather:

SOUL-SCORN

My Soul said, "Art thou dead?
The chasm of night is riv'n;
What dost thou see?" I said,
"The full-fired fires of heav'n."

"Look not, but see," he said.
I said, "I know not whether
They are the hosts of God
Clashing their spears together.

"So bright the stars appear
Their splendour smokes in heav'n.
I think indeed I hear
Their distant voices even."

He said, "See not, but know."
I said, "I cannot see;
I think perhaps they go
To some great victory."

He said, "For ever they go,
Still onward, on and on;
And that is why they know
The victory's clarion."

I said, "I am too weak
To do more than I must."
He said, "Then cease to seek
And perish in the dust."

CHAPTER VII

COONOOR, HOME, 1893–1895

In April 1893 my appointment as Staff-Surgeon at Bangalore ended. We then took three months' leave to Coonoor, near Ootacamund, where we stayed in the Hill Grove Hotel. This was the first time (except for the morning visit mentioned on page 94) that I had ever been in an Indian hill station. The sudden change from the burning plains to the almost English climate is an experience which is given only to the "Anglo-Indians," as we are called in England. From dust, heat, flies, mosquitoes, and aridity we are lifted in about four hours' journey by tonga into valleys of flowers, way-side streams, and heavenly coolness. On this occasion, the only thing which dashed our joy was the cruel flogging of the little ponies which drew us up the road, generally at a gallop. One of them had sores and abrasions, and I proposed to report the matter to the local branch of the Society for the Prevention of Cruelty to Animals; but learnt that the chairman was the owner of the tonga-service!—a characteristic conclusion.

After about a week's rest and recuperation, I commenced indulging in day-long rambles round Coonoor—starting out after breakfast, walking nearly all the time, and returning for lunch at 4 or 5 p.m. so hungry that I was generally ready again for dinner at 8 p.m.! I followed main roads for a distance, and then struck off across hills and valleys in all directions. Taking my note-books in my pocket, I generally meditated "In Exile" as I walked, though I found that after about a ten miles' walk my brain was superlatively content but almost unable to meditate seriously about anything. After noon, at that season, small rainstorms were frequent, and I allowed myself to be wet through (drying again as I walked home) with impunity, and this kind of physical worship did me much good—combined as it was with nature-worship.

During one of these walks, on returning from the mountain overlooking Ootacamund, I was followed for nearly a mile along a steep hillside by one of the half-wild Nilghiri buffaloes,

bellowing and threatening to charge, about twenty yards on the slope above me. I escaped by pretending to take no notice of the beast whatever—a very excellent way of avoiding more dangers than one!

Besides the walks, we enjoyed long drives in our pony-cart, drawn by our dear old Waler pony Gameboy. There was much pleasant society; and one day we gave a picnic to about thirty people in a beautiful valley which I had discovered east of Coonoor, where Appoo and our servants laid out a fine repast. Some of us rode to the banquet, and then galloped an improvised race up the valley. Lovely days of bright suns, purple shadows, rainbowed mists, health, and high thoughts. I have often wondered by what stupidity, during my twelve years of service in India, I had never taken leave in these beautiful mountains before.

It must have been in June 1893 that we left Coonoor for Secunderabad, passing through Bangalore *en route*—the journey was terribly hot. We arrived at Secunderabad, after about a week, late one night.

My appointment was merely that of temporary medical officer to a native regiment (the 20th Madras Infantry) at Tarbund Lines at Secunderabad—a "come-down" after the Staff-Surgeoncy at Bangalore; and, though I now had twelve years' service and was a major, I was still not fully paid. We had a miserable little bungalow, too, and were not over-happy; but the climate suited us better than that of Bangalore.

Secunderabad was (and is) a large military station containing many regiments of British and Indian soldiers. It is situated about eight miles from the great Indian city of Haiderabad, on a vast rolling plain upon which small hills consisting of heaps of big boulders are scattered, with little lakes interspersed between them—now, during the rains, quite beautiful, green, and sparkling. Here all day I toiled hard trying to find Laveran's malaria parasites in the blood taken from the pricked fingers of patients; but without success, because I was using an elaborate technique of my own, which was, well, too elaborate!

On the occasion of the great Mohammedan festival, the Moharram, the Maharaja asked us, with most of the other British residents, to watch the procession file past his palace—a remarkable mediæval spectacle of knights in armour seated on elephants and horses followed by their retainers!

We had not been here many months when I was given a permanent appointment as surgeon to the 19th Madras Infantry

at Berhampur. It was a trying journey with two small children, involving crossing the Godaveri at night and taking ship at Rajamundri for Gopalpur on the coast, close to our destination ; and I had to pay the fares for my family because I was " proceeding to a better-paid appointment " ! We had to spend one night in the railway carriage in a siding, devoured by mosquitoes ; but a railway official and his half-caste wife put us up with great kindness for another night. On the voyage to Gopalpur we met two officers of the regiment, Captain Thomas and Lieut. Knox, belonging to my regiment. I met Knox again during the Parliamentary visit to Russia in 1912 (page 509), and found him to have become Military Attaché to the British Embassy, and he is now General Sir Alfred William Fortescue Knox, K.C.B.

Berhampur was a charming small station on a verdant plain of grass, rice-fields, palm-trees, and banyans, some miles inland from the sea, and with a beautiful little range of mountains about five miles to the west. We hired a nice bungalow from an engineer officer on leave, including a great deer-hound and a black leopard in a small wooden cage—a thing which we did not like. All the British people there were extremely nice. The adjutant of the regiment was Captain Rowe, whom I had met on the way to Pakoko in 1889. His charming Irish wife became a great friend of ours—she sang beautifully ; but he was not well, and died of abscess of the brain in England a little later. The Padre was a Mr. Whiteley, an excellent man, whom I offended by thoughtlessly refusing to give alms to a stout English tramp, a matter which has been on my conscience ever since ! Another friend was an engineer, whose name I have forgotten, but with whom on day I walked to the hills just mentioned and ascended them. We lunched high up the mountain side, and the hillmen gave us some fermented cocoa-nut milk in a palm-leaf cup, which we found to be distinctly strong. A shepherd boy singing on the summit gave me the first idea of my verses, " The Shadow of the Mountain," which I expanded afterwards into my " Indian Shepherds " [117].

It was a happy time at Berhampur, during which I did little work except my small regimental duties. At Christmas (1893) the station enjoyed a fancy dress ball, at which, I remember, a big, stout man called Eve appeared crowned with laurel as a Roman Emperor. Early next year I obtained another year's furlough in England, and we left for home by the s.s. *Clan Macpherson* in February 1894.

We arrived in England in March 1894, and stayed with my wife's brother, Mr. Cazalet Bloxam, at Hampstead, and then with my mother at Surbiton. Immediately on arrival I took steps regarding my studies on malaria—as will be described in Chapter IX ; but at the same time I occupied myself with two literary projects, first my romance, *The Spirit of Storm*, published in 1896 [111], and secondly, my romance finally called the *Revels of Orsera*, which was based on my drama, *The Deformed Transformed* [110], but was not published until 1920 [122].

My friend, Frank Aston-Binns, however, now persuaded my wife and myself to go with him and his father, mother, and sister to Switzerland. We left about the end of July 1894, and travelled via Calais and Thun to Kandersteg in the Bernese Oberland. There rose the great mountains before us—not, as in the Bolán Pass, huge masses of decaying rubble, nor, as in the Nilghiris, rounded slopes of verdure, but real masters of the world, glittering in mail of ice and snow and mantled with pine forests. And what an air after the heats of India.

The Reverend Mr. Aston-Binns (pages 21, 32) was a short, thickset man with a large head, broad forehead, a magnificent grey beard, and an expression both pragmatic and humorous. He was a fine preacher, looked like John Knox in the pulpit, and was always delighted with the delight of others. And what a woman was his wife, who had been so good to me when I was a boy at Ryde, and whom I called my " Mater " ; made up of nothing but kindness and affection, and always receiving what she gave. Even then she was suffering from her first slight stroke of paralysis and required to sit, always smiling, in the sun. Her daughter Mary was five years older than her son, and ruled us younger people with some discipline. My friend Francis (Frank) was born at Warwick on 3 May 1859 (pages 21, 32), was brought up at home and (?) at Rugby, and was then obliged to go abroad for threatened consumption. But he became so expert at French, Italian, and German that he obtained the Taylorian Exhibition in French at Oxford in 1883 and the same Scholarship in Italian next year. He was at Balliol in the time of Jowett, later tutor to Lord Acton's son, and finally, in 1892, on the staff of Sherborne School for modern languages, where he was now. His health was much improved since he had taken to climbing, at which he was a master ; and he was now a brilliant young English don. His friend, Dr. Wherry, of Oxford, author of *The Climbing Foot*, met him at Kandersteg, and they proposed to take me up to

various summits. But though, so far as the walking went, I could climb to any height, yet, like my father, I could not endure a precipice. Even to look up at one made me feel sick. Chiefly imagination, I think; but they gave me up, and I took to rambling and fly-fishing in the Kandersteg streams instead —much annoyed, because I would have given anything to scale those sublime peaks.

My mind was full of Astrella and Zozimo; so much so that my wife declared I had been shouting the words of the Spirit to Astrella in my sleep! I determined to visit the scenes where they had lived (in my imagination), and which was evidently in the centre of Europe, the valley of Andermatt, where all the rivers rise and all the ranges converge. I must go alone. So, after three weeks of Kandersteg, I set out on foot—but Aston-Binns went with me to the top of the Gemmi Pass. On the way we had a diverting incident. We were going up the steep road at the climber's pace of about forty-five steps a minute only, when a party of Americans—men, ladies on donkeys, and drivers and guides—rushed past us at full speed and looked back rather scornfully at us as they went. Presently we found them all seated fanning themselves on a bank, and we plodded coolly and comfortably past them in our turn. In another ten minutes they overtook us again with a kind of triumphant rush. This time they made a more determined effort, for it was quite a quarter of an hour before we passed them, utterly collapsed, for the last time. As we receded up the ascent we heard a resolute lady call out, "My, Jack, I do believe those Britishers are going to beat us after all." In fact, we had finished our lunch in the inn at the top of the pass when they arrived, wiser but sadder people. Here, strangely enough, I met again that good man, Colonel Cadell, V.C., C.B., formerly Chief Commissioner of the Andamans (page 69). My friend now left me, and I proceeded alone on foot or by diligence, up the Rhone Valley. In a Swiss church on the way there are—or were—many quaint pictures illustrating the New Testament, in which all the personages are clad in seventeenth-century costume, wigs included.

Arrived at the Andermatt Valley in two or three days, I stayed at the inn at Hospenthal and immediately recognised in the old tower the site of the palace of Count Reichenfels (now disappeared). There stretched the small and lofty plain of Andermatt (= Orsera), where the trial of Lelita's suitors and the subsequent tournaments were held. I went to the chapel and saw the skulls of the Cardinal and of Müren (also now

vanished !), and photographed the whole scene in my memory. One day I walked up the St. Gothard Pass and caught a large number of trout in the small lake at the top by means of a Devon minnow, which these fish had evidently not seen before (this pond discharges both into the Mediterranean and the North Sea, I was told). Then I moved to the inn at Realp on the way to the Furka. Below me had been Count Reichenfels' inn, where Capon and Pompilia lived, where Zozimo met the Mysterious Stranger, and where the final catastrophe befel. Then with a guide I climbed the Ochsen Alp and found the cottage where the terrible Morva and her twin children, Astrella and Zozimo, dwelt (vanished too !); and climbed the Tiefen Glacier and the rock that looks over into the Rhone Valley, where Gangogo flung down the great Cardinal; and lastly, the dreadful summit where these twin children, the Divine and the Deformed, died together in each other's arms [116]:

> On icy peaks at death of time
> Rings out that awful end sublime.
> Black all the mountains, black the air,
> And black the anguish dying there
> In music that no menace mars
> Beneath the pale empassion'd stars.
> There Life in apologue is shown ;
> And Love and Death in undertone
> Make Wisdom wiser than alone.

I saw all these things and people myself; and then, with my guide, ran down the steep path to Grimsel and so (still running most of the way) along the Valley of the Aar to Innertkirchen and then to Grindelwald—to which our party had moved meanwhile.

> The visions of the Soul, more strange than dreams,
> Out-mystery sleep.

There in Grindelwald we spent some more happy weeks. I remember the enormous masses of the three great mountains before me, but, perhaps just as vividly—so mean is the memory—a singular signboard posted at the entrance to an ice-grotto (and the matter found its way into *Punch* some time later). The inscription put up in English by the worthy Swiss proprietor was: "Picnic Parties promptly Executed on the Spot" !

Shortly after returning home in September 1894 we went to live with my mother at Surbiton, and I wrote my *Spirit of Storm*. This is a parable of *unreasonable wrath*, all the scenes and incidents being arranged round the main theme so as to form as perfect a construction as I was capable of. It

was meant merely to be a *tour de force* of invention, nothing higher. The idea had occurred to me when I was writing *The Child of Ocean*, of which some of the subsidiary motives were now to be elaborated in greater detail—as composers often elaborate theirs. Warned by previous experiences, I had mixed more of the jam of convention with the powder of art in this work; and it was therefore speedily accepted (March 1895) by Methuen & Co., who had, indeed, asked me for a book five years previously on their reading *The Child of Ocean*; and it was published in 1896, after I had returned to India, the conclusion requiring some changes, which I made at Secunderabad that year. But one publisher's reader of the comfortable, commercial stay-at-home type, had actually suggested that the end of the book (which was "happy," though not conventional) should be given yet another ending of an appallingly mawkish type. I wonder if a Greek critic could ever have asked Homer to change the end of the *Iliad* by allowing Achilles and Hector to be reconciled, with tears, over the body of Patroclus!

The book had some very favourable reviews, especially one by Mr. Stephen Lucius Gwynn (M.P.) in *The National Observer*, 14 November 1896. The whole edition (2,000 copies) was ultimately sold out, and I received altogether £17 7*s*. 11*d*. for it from the publishers, on a 10 per cent. royalty. I add Mr. Gwynn's review, because it will save me the trouble of describing the book. He himself told me it was written by him—I met him at the University Club, Liverpool, about 1905.

"Mr. Ross has written a very notable book. He is nothing if not ambitious, since he takes George Meredith for his model in style and chooses his subject after the fashion of Victor Hugo. But in order to discuss the book at all adequately it is necessary to sketch the plot. John Chesham, Governor of St. Vincent in the West Indies, married Diana Sinclair, the most beautiful woman of her time, whom he passionately adored. But illness forced her to leave him, and their son was born in England. Meanwhile one Smith, a disreputable cousin of Chesham's, next heir after the boy to the entailed Chesham estates, worked upon the man's impetuous nature with lies about his wife, which were too easily credited. Mrs. Chesham, returning to her husband, was openly charged with disloyalty; and to vindicate herself she sent to England for the boy, whose foot bore a particular mark reproduced in the Chesham men for generations. At Port Royal the boy Percival was met by

Lord Tringham, Chesham's closest friend and an old lover of Diana's, who embarked with an escort on the trading-vessel *Deianira* to conduct Percival to St. Vincent. But on board the same vessel was Percival's father, who in a fantastic fit of jealousy had made it appear that he was drowned at St. Vincent, expecting that his wife's conduct after his disappearance would justify his suspicions. He had gone secretly away to capture and meet the lad, intending to kill him if the mark did not appear on his foot. Lord Tringham's unexpected presence forced him to hide in the ship's hold, and fever came on him. Upon the ship was also the rascally cousin, whose object was to make away with the lad, and he was aided by the master, Boalth. But the real owner of the *Deianira* was Ruth Donderbass, a formidable negress who protected Percival. Out of all this arose strange adventures in the first part of the voyage before the vessel reached San Domingo. It was in the days of the French Revolution and the black rebellion. The *Deianira*, becalmed in a dense fog, was boarded first by planters escaping from the rebels, then by the crew of Ruth Donderbass's larger vessel, a slaver with a full cargo of slaves aboard ; and finally by the blacks pursuing the planters in a flotilla of boats. At the same time there lay in the harbour a British frigate with Mrs. Chesham on board it, and with her Captain Wilson, the man suspected. Lord Tringham learnt this from Mr. Boalth, and at the same time heard the report of his friend's death. He therefore believed the worst of Diana, and sent a message by a boat refusing to give up Percival until Captain Wilson left her, at the same time asking help from the frigate. Before the help came a desperate fight had been fought in the dense fog, planters, slavers, and rebels engaging in a three-cornered conflict, interrupted by three great tidal waves, the forerunners of hurricane. Towards the close of the fighting the fog lifted, but the *Deianira's* helm was shattered, and the wind, springing up, carried her again into the fogbank, and she was deserted by all parties, who felt the storm coming. Percival, in the confusion, had been seized by his father and carried into the hold.

" At this point there is a complete break in the story. There has been, so far, a crowded canvas, much variety of incident, much dialogue, a good deal of comedy, and then the terrible

drama of battle. Chesham has not appeared; his presence on board is a mystery, though hinted at again and again. The central figure so far is Lord Tringham, and admirable he is; a splendid portrait of the fine gentleman soldier who adorned that period: brave, courteous, scholarly. Ruth Donderbass the negress is a picturesque and formidable personage; the minor characters, soldiers, sailors, and the rest, are excellent; and the boy Percival is as good as Lord Tringham: a precocious little gentleman of ten, with sword by his side, but delightfully gentlemanlike and delightfully boyish. Now the stage is cleared, or crowded only with corpses; and the rudderless vessel drives into the tornado with only three living souls on deck: the boy, his father, and a negro-leader, Biassou, who was forced to save his life by shooting his own daughter. But winds and waves are the real actors. All the terrible phenomena of a tropical whirlwind are described with an imaginative power which suggests Hugo; and on the deck, in the dark, the boy crouches and hides, with corpses knocking against him, and his father in the delirious fit of fever pursuing him to kill him. The childishness of his terror is infinitely touching; for the brave nature of the boy is consistent throughout, and struggles against the irresistible. Finally, the electric fires, corposants, show the son his father's face: the face of a madman. Next moment the light forms over his own head and he is captured. But Chesham cannot kill him, and the boy goes to sleep in his arms. So in this strange repose they run into the centre of the storm, for at the heart of every hurricane there is calm. The correspondence between Chesham's mood and the tempest outside him is plainly designed. In this calm comes awakening; the boy and his father talk as friends. Then comes revelation. Smith the cousin, who has been wounded to death in the fight, crawls on deck, hearing voices. He sees the boy and confesses his villainy before he dies. So Chesham knows that he has been a fool, and worse. From this to the end—for the tornado in its circling course leaves the ship where it took it first, and all the main actors assemble for the last scene on the sinking vessel—the psychology is surprising. This man, in whom everything is 'a stampede of wild nature,' will not own himself in the wrong, till the better side of him is touched by the strangest means. Yet it is hard

to point to anything, and say this is not right. But one wishes that Mr. Ross had kept clear of studying insanity. Chesham is irrational to the verge of madness; and the passage of delirious ravings in a kind of antiphon between him and the wretched Biassou seems to strike a false note.

"In the matter of style, no one can copy Mr. Meredith with impunity. Mr. Ross has a good many laboured pages, especially in the first few chapters; for instance, the description of Chesham's character. But he aims high, and hits oftener than he misses. Still, in his next book (for which many readers will look with expectation), he ought, if only as an exercise, to confine himself to the English language. 'Exsplendescent' is only one of perhaps a hundred new mintings in this small volume. But Mr. Ross has scholarship, eloquence, and poetry of style, he has a remarkable power of presenting character, and above all he has imagination enough for half a dozen novelists."

I had never read Meredith, I am ashamed to say.

I also finished early in 1895 the romance form of my Swiss drama, *The Deformed Transformed*; but it did not suit any publishers, and was not brought out until twenty-five years later [122].

The winter turned bitterly cold on 5 January 1895, and we had some skating at Surbiton. On 20 March I won the Parkes' Memorial Gold Medal with a prize of seventy-five guineas given by the Army Medical Service at Netley Hospital, for an essay on malaria (page 129). Altogether I must have worked hard during my "holidays," especially as I was obliged to do much reading at the British Museum and elsewhere for all these works. In consequence I became rather ill with continued influenza, and was not cured until I went to stay with Aston-Binns for a few days at Sherborne School. He was a great climber, but a poor rider, though he desired to ride better; while I had "a bad head" for climbing but feared no horse. He put me on a terrible horse with a mouth of iron, and I was so feeble with influenza that I nearly fell off on the other side. Then we went away for two hours over the country, my brute bolting most of the time. But when we returned I had no more influenza! We lived in a part of Sherborne Abbey which had been separated from the church, but which is now being restored to it.

This was the last I saw of my friend. I returned to India shortly afterwards; and he and his guide were killed while "taking a stroll" on the Aiguille de Charmoz, near Chamonix, on 16 September 1898. Before this he had been much upset by the death of his mother, my dear "Mater," at Chamonix, on 30 August 1895. His father married again, but died at Geneva on 11 June 1899, on his way to Chamonix to dedicate a pulpit there in the English Church to the memory of his first wife and son. Miss Aston-Binns died at Reigate on 13 May 1921.

Before I left India we were all shocked by the news that my brother Claye had been killed in action near Chitral in India. He was born in India on 21 December 1861, and was educated at the Isle of Wight College at Ryde and then at Stubbington School, in Hampshire. In 1881 he entered the Army (Gloucestershire Regiment), joined the Indian Staff Corps two years later, and in 1885 was attached to our father's regiment, the Ferozepur Sikhs, now called the 14th Bengal Infantry, and obtained his captaincy in 1892. He was in England with me in 1881 and again in 1888—when I saw the last of him. On his return to India then, his regiment, or a part of it, was stationed at Gilgit, in Kashmir; and when the Chitral trouble commenced in 1894 he, being in command at Mastuj, near Gilgit, was ordered by Dr. Robertson, the British Agent at Chitral, to proceed there with as many of his Sikhs as possible. As fighting had already commenced at Chitral and the whole country was in a ferment, this was a dangerous move, but my brother was obliged to push forward, as another detachment of Sikhs under Lieut. Edwardes was isolated some marches nearer Chitral. On the morning of 7 March he left Mastuj (sixty miles from Chitral) with Lieut. Jones, ninety-six men of the 14th Sikhs, and other details, and next day proceeded with Jones and sixty-two men through the narrow Koragh Defile. Here they were attacked by about 1,000 ambushed tribesmen, who fired and rolled rocks down the steep hill-sides upon them. After heavy fighting, they were obliged to take refuge in two caves. They remained there the whole of 9 March, but on the 10th tried to cut their way out. My brother was hit in the head, first by a stone and then by a bullet, and was killed; but Jones and fifteen men (most of them wounded) finally escaped. The other wounded, who were left behind in the caves, gave themselves up after eight days on promise of being spared; but they were all massacred by the treacherous Chitralis, with the

exception of one man, Ghanda Singh, who escaped to tell the tale.

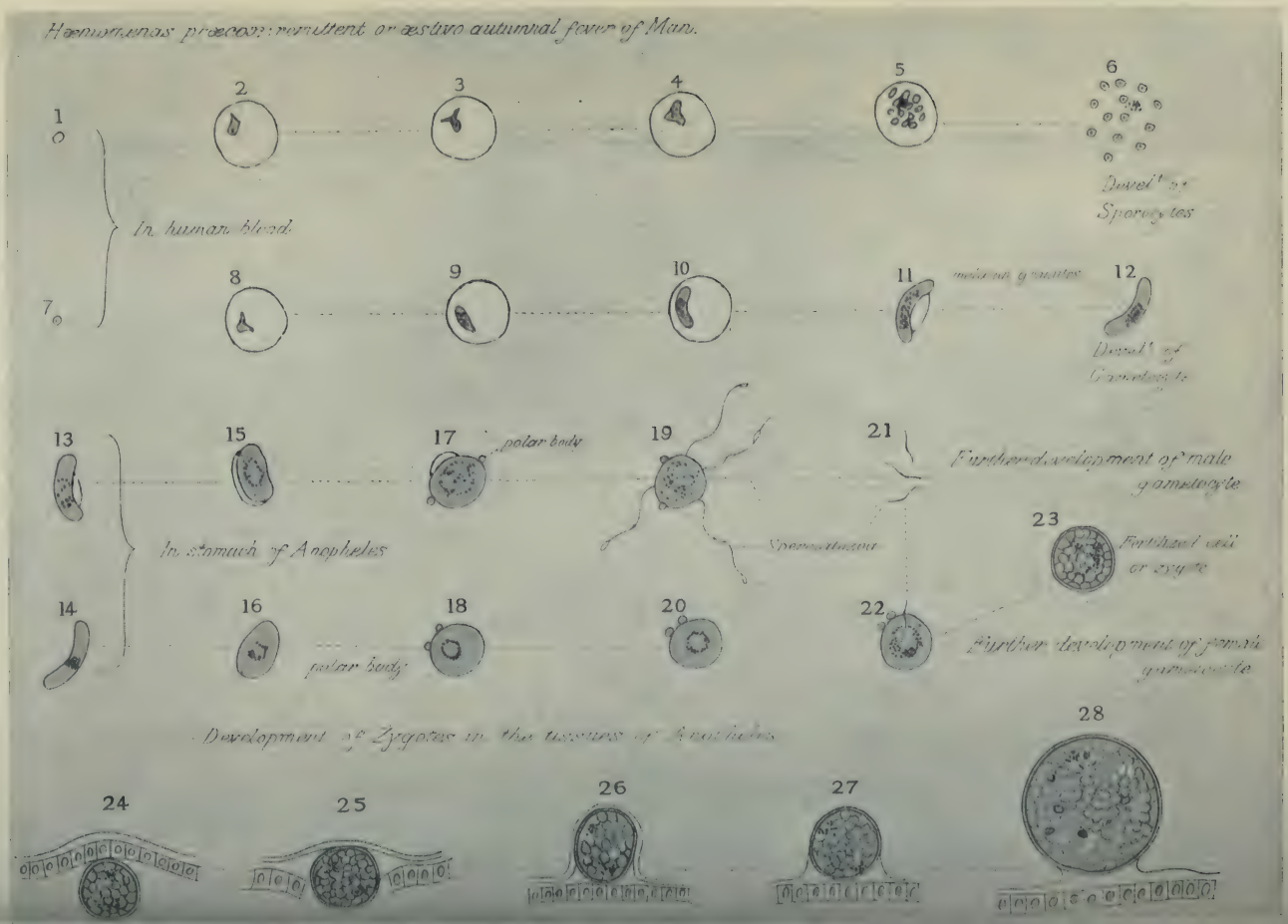
My brother (whose full name was Claye Ross Ross, the middle name having been given to him by a mistake at his christening), was afterwards blamed for not remaining at Mastuj, in spite of his orders to proceed to Chitral; but he is amply vindicated by the official reports, of which I possess copies. It was not then thought that the Koragh tribesmen would oppose them, and besides, he was obliged to follow in support of a small advanced detachment which was in great danger (and was afterwards treacherously captured by the enemy). The relief of Chitral is a matter of history.

Claye was a mild, studious, thoughtful boy and young man, who was developing rapidly into an expert in military matters. I thought by his letters to me that he would soon take to writing on these—just as our brother Charles (now Major-General Ross, C.B., D.S.O.) has done. He was also interested in philosophy; but I had seen little of him since we were together in 1881.

My son, Ronald Campbell, who was destined for a similar fate, was born at Surbiton a month earlier, on 11 February 1895. On 28 March I departed again for India, leaving my wife and three children at home. My age was thirty-eight, and I had attained the rank of Surgeon-Major in 1893, after twelve years' service.

PART II

MALARIA


Haemamœbas præcox: remittent or æstivo autumnal fever of Man.
1
2
3
4
5
6
Devel.t of Sporocytes
In human blood
7
8
9
10
11
mâle ou gamète
12
Devel.t of Gamétocyte
13
15
17
polar body
19
21
Further development of male gamétocyte
In stomach of Anopheles
Sporulation
23
Fertilized cell or zygote
14
16
polar body
18
20
22
Further development of female gamétocyte
Development of Zygotes in the tissues of Anopheles
24
25
26
27
28

DESCRIPTION OF PLATE II.—LIFE-HISTORY OF THE *PLASMODIA*

(From article by R. Ross and R. Fielding-Ould, *Journ. Micros. Science*, 1900)

FIGS. 1–6.—ASEXUAL LIFE OF *Plasmodium falciparum* (OR *Hæmomenas præcox*) IN HUMAN BLOOD.

FIG. 1.—Spore.
FIG. 2.—Spore in red corpuscles.
FIGS. 3, 4.—Growth of young parasite in red corpuscle.
FIG. 5.—Parasite matures and forms spores.
FIG. 6.—Corpuscle bursts and spores are scattered in the blood-fluid.

FIGS. 7–12.—SEXUAL LIFE OF *P. falciparum* IN HUMAN BLOOD.

FIG. 7.—Spore.
FIGS. 8–10.—Growth of young parasite in red corpuscle.
FIGS. 11, 12.—Mature sexual parasites (called crescents).

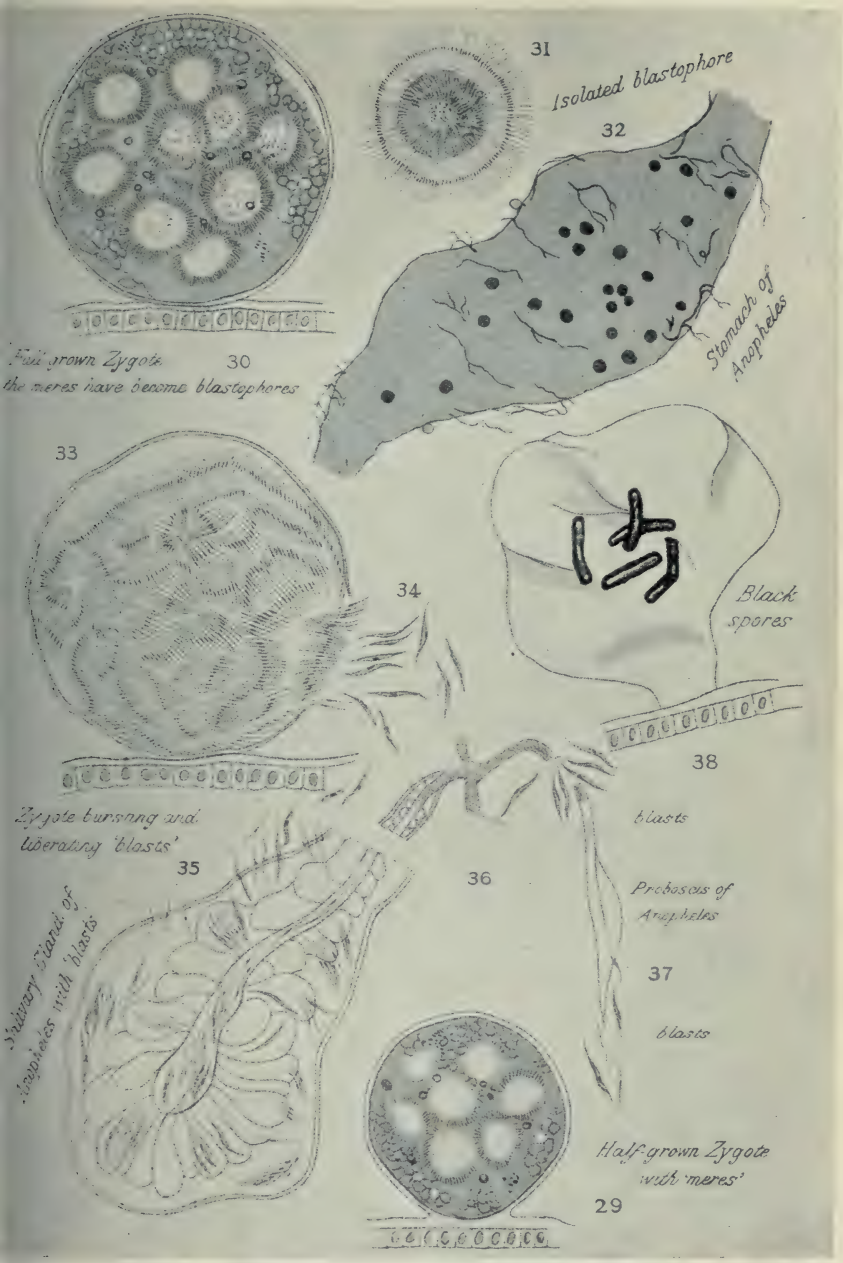
FIGS. 13–23.—CHANGES OF THE SEXUAL FORMS OF *P. falciparum* IN THE BLOOD CONTAINED IN STOMACH-CAVITY OF *Anopheles*.

FIGS. 13, 15, 17.—The male crescent swells up and becomes round.
FIG. 19.—It emits sperms (formerly called flagella).
FIG. 21.—Free sperms.
FIGS. 14, 16, 18, 20.—The female crescent becomes round.
FIG. 22.—A sperm enters the female cell.
FIG. 23.—The fertilised female cell, or zygote.

FIGS. 24–38.—DIAGRAMS OF THE GENERAL DEVELOPMENT OF THE PLASMODIA IN THE TISSUES OF MOSQUITOES.

FIGS. 24, 25.—The zygote breaking through stomach-wall of mosquito.
FIGS. 26–28.—Zygote attached to outer coat of stomach and growing in size.

Fig. 29.—Zygote attached to outer coat of stomach and growing in size.
Fig. 30.—Mature zygote (oöcyst) containing blastophores.
Fig. 31.—A single blastophore with blasts attached by their ends to a central sphere.
Fig. 32.—Stomach of mosquito with mature zygotes attached (low magnification).
Fig. 33.—The central sphere of the blastophore is absorbed, leaving the mature zygote packed full of blasts.
Fig. 34.—The zygote ruptures and the blasts (protospores, sporozoites) escape into the mosquito's blood.
Fig. 35.—The blasts enter the mosquito's salivary gland.
Figs. 36, 37.—They pass up the ducts of the gland and escape by the central stabbing lancet in the mosquito's proboscis into blood again.
Fig. 38.—The capsule of an empty zygote containing some "black spores."



LIFE-HISTORY OF *PLASMODIA* (*continued*).

CHAPTER VIII

THE PROBLEM

As the next four years of my life were devoted almost entirely to investigations on malaria, I must now make an effort to describe the subject in language suitable to the lay reader. It is the main object of the book to interest him in the matter. The lay reader rules the world. We doctors are only helots. We advise, he snubs us; we advise again, and perhaps some day he listens to us! But I hope also to win the suffrage of men of science by an accurate history of the events.

Malarial Fever is the most common of infective diseases almost throughout the tropics, and occurs during the summer even in temperate climates. In the tropics it causes roughly about one-third of all the attendances at hospitals; and about one-third of the entire population suffers from it every year. Although only about one case in several hundreds dies of it, yet the prevalence of the disease is so enormous that the total aggregate of the deaths directly due to it, and still more indirectly due to it, is colossal. Thus it has been officially estimated that in India alone something like 1,300,000 deaths were caused by it in years during which it was not exceptionally prevalent; and at the end of last century out of about 300,000 men in the army in India, 100,000 were admitted into hospital every year for malaria. Similar statistics are given for almost all hot countries except Egypt and some other isolated tracts; in Greece and round Rome the disease is a curse; and it has prevailed in Europe as far north as Holland, England (in 1917 and 1918), and Sweden. In addition to the actual frequency of infection most cases tend to relapse even for years after the original attack; and lastly malaria has a great political influence because it haunts especially the most fertile and well-watered parts of the earth. Probably malaria has done more than anything else to prevent the settlement and civilisation of the vast tropical areas which would otherwise be most suitable for the human race. All knowledge connected with

malaria is therefore of the very first practical importance for mankind in general.

The disease itself is an *intermittent fever*; that is, a fever characterised by regular recurrences every day, or every two, or three days (called *quotidian*, *tertian*, and *quartan* fever respectively). The recurrences generally begin about midday, often with severe chills and shivering (*rigor* or *ague* attack), succeeded by high temperature reaching 105° F. or more. This fever continues for variable periods up to twenty hours or more, and then abates with profuse sweating. Sometimes however, in quotidian, or even in tertian, fever, a second attack commences before the preceding one has quite abated, when we have what is called a *remittent fever*. In badly treated cases these recurrences may continue for weeks and sometimes, with intervals of comparative quiescence, for years. I remember that my father had a typical relapse of malaria just as we were sitting down to lunch one day in 1888, eight years after he had left India; and relapses after much longer periods are known to occur. On the other hand many cases recover completely after the first few attacks, and most cases recover naturally about the third, fourth, or fifth years of infection. In 1900 Robert Koch discovered that most native children in malarious areas in the tropics are infected shortly after birth and remain infected, or are reinfected over and over again, until they reach about fifteen years of age, when they appear to throw off the infection and become partially *immune* (p. 422). A single attack of the fever causes considerable *anæmia*; and when the attacks recur frequently the patient may become very anæmic and weak, though he tends to rally during the periods of quiescence. The large gland called the *spleen*, which is situated just above the lower edge of the ribs of the left side, rapidly increases in size when the attacks are numerous (if no quinine be given), and sometimes becomes extremely large (*splenomegaly*). The disease is what is called a benign one, but fatal cases sometimes occur early in the infection in certain dangerous varieties called *pernicious attacks*, amongst which the famous *blackwater fever* is the most common cause of death. Probably early deaths from malaria are the principal cause of child mortality in tropical countries; but even when patients do not die from the disease they may be kept for years in a weak state and unable to work, and may be carried off at the beginning of the cold season by dysentery or pneumonia. Of course the general use of the specific remedy, quinine, modifies much the progress of the illness, which must have been a very serious

matter in the days before quinine was discovered. During the war a number of cases were accidentally infected with malaria and were not treated by quinine early enough owing to the fact that the infection was not discovered in time ; and a large proportion of these men died—showing the potential severity of the disease, especially in the form called *malignant tertian*.

Necessarily all nations living in warm countries are only too well acquainted with malaria, and the ancient Greeks and Romans were no exception to this rule. After I visited Greece in 1906 (page 496) I enunciated the hypothesis that malaria may have entered that country for the first time during the earliest ages just as it entered Mauritius in 1866 in the memory of people now living [83] ; and subsequently Mr. W. H. S. Jones of Cambridge gave us a laborious and exhaustive study on the subject made in the light of our present knowledge [89]. There are few references to malaria in the earliest literature, but many in later writings. For a summary of this interesting theme see my book [91].

It is well known that the ancients recognised the quartan, tertian, quotidian, and semitertian (probably malignant tertian) varieties of malaria, and many of its accidents ; and that they were acquainted with its seasonal and local variability, and, above all, with its frequent prevalence near marshes. This last point is most germane to our present part of the subject.

Thus Strabo (first century B.C.) says that Alexandria, in spite of its site, was free from marsh-fever even in his time. " It is to be inferred from this," Mr. Jones says, " that damp places were generally known to be unhealthy, so that exceptions to the rule were noticed by observers as remarkable phenomena."

For ancient Italy we have a similar story, namely, that references to paludism are scanty in the earlier writers but very abundant in the later ones. Many areas round Rome, now scarcely habitable, were the homes of great and prosperous peoples in the prehistoric period, and later were full of the country villas of rich Romans.

Of course the Roman writers, both medical and non-medical, were acquainted with the leading facts about paludism mentioned above. There are also several passages referring to *mosquito nets*, called *conopeum* by the Romans, after the Greeks (our word canopy). Herodotus first noted with surprise the use of them in Egypt ; and they are referred to later in Varro (*De Re Rustica*, 2, 10, 8), Horace (*Epodes*, 1, 16), *Propertius*

(3, 11, 45), Juvenal (6, 80), and Paulus Silentiarius (*Anthologia Palatina*).

Little was added to our knowledge during the next 1,500 years; but about 1640 the inestimable boon of *Cinchona bark* was introduced into Europe. The Countess d'El Cinchon, wife of the Viceroy of Peru, had been cured of fever by means of it in that country, where it had been discovered by the Indians near Loxa (?); and she was wise enough to send it home to Europe. The use of it, after many checks, gradually spread; and in 1820 Pelletier and Caventou extracted the alkaloid quinine from it. But the discovery of this specific has not only proved to be a blessing for the treatment of untold millions of human beings, but also enabled Morton (1697) and Torti (1753) to separate the malarial fevers, which are cured by it, from those upon which it has no influence, and by this means to differentiate and study the symptoms of the former. Morton also recalled the old hypothesis of the marsh; and this was amplified by Lancisi, who repeated the ancient sayings of Varro [1] and Palladius [2] in greater detail in his book *De Noxiis Paludum Effluviis* [3]. He stated that fevers disappear after drainage and attributed the poison either to inorganic or to organic emanations from the marsh. He studied mosquitoes, and even (?) suggested inoculation by them as a possible means of infection—though he also thought that their larvæ foul drinking water.

It is now apparent that the world had been gradually becoming aware during centuries of the paludic nature of malarial fever, not by direct experiment or even by investigation, but by a kind of subconscious experience based on public observations. In Italy especially, where of all civilised countries the disease was most prevalent, this process was most apparent. More than this, by similar general observation, the good effect of assainment of marshes had become equally notorious there. Thus, as early as 1667, Doni wrote a work called *De Restituenda Salubritate Agri Romani*; and references to a succession of works carried out on this principle, which I now call the principle of Mosquito Reduction, were given by Celli (1901). At the same time efforts were made by many observers, such as Morton, Lancisi, Lind, Pringle, to explain the paludic connection; and these resulted in the hypothesis of the *paludic miasma.* This was supposed to be some kind of infecting emanation from stagnant water, either chemical, or, as Lancisi suggested, organic; but in no cases, apparently, were experiments made to test the point. Later, when it was observed that malaria

may sometimes occur where there is no marsh, the hypothesis of the paludic miasma was extended to become that of the *telluric* miasma, according to which the poison exists not only in marshes but anywhere in suitable soil, from which it rises at night or when the soil is disturbed. This speculation, for it was nothing more, is not quite dead even yet, though the observation which originated it is easily explained otherwise. The word "malaria" (*mal'aria*, or bad air) is derived from it. Many writers attributed the disease to various marsh-growing vegetable organisms. In 1862 Salisbury in particular, after considerable study, blamed a kind of *Palmella*; and after 1878, a number of Italian workers, Lanzi and Terrigi, E. Klebs and C. Tommasi-Crudeli, thought that they had actually incriminated certain fungi or bacteria, which they said swarm in malarious places, occur in the blood, produce spores before each paroxysm of fever, and cause similar infection in animals. These findings were even confirmed by Marchiafava and other Italians, but have now been completely discredited (page 397).

Meantime, however, other light had been thrown on the subject from various quarters. As long ago as 1846 Rasori made an extraordinary prediction on this subject. "For many years," he said, "I have held the opinion that the intermittent fevers are produced by parasites that cause the successive paroxysms of fever by their reproduction, which occurs periodically more or less rapidly according to their species"—and this has proved to be exactly true. But it was only speculation, and modern pathological science demanded strict microscopical and experimental evidence. I had the same idea about 1883.

In 1847 H. Meckel discovered innumerable black granules in the blood of an insane patient [4]; and the discovery was subsequently confirmed and amplified by other observers—the granules (Plate II) now being known under the name of the *paludic pigment*, or *melanin*, or *hæmatozoin* (Sambon). For a long time they were thought to be due to a chemical action of the paludic miasma on the red cells of the blood. This, however, was discredited in 1878, when C. Gerhardt proved that healthy persons can be infected by the inoculation of blood of patients suffering from paludism. His experiments were afterwards verified by many workers, and demonstrate (apart from the subsequent microscopical discovery of the parasites) that the disease is not due to any gaseous emanation from marshes, but is a true infection by some living virus.

Now came a great discovery. In 1878, A. Laveran, a French army surgeon, commenced his studies of the subject at Bône

in Algeria, by following up the granules of pigment, already referred to, in the blood of living patients. He was struck by the fact that they were frequently contained within the red corpuscles in small cells possessing active amœboid movements (Plate II, figs. 2–4, 8–10); and, finally [6], on 6 November 1880, at Constantine, he detected the sperms (microgametes, flagella) issuing from the male cell (flagellated body, ib. fig. 16). Though at the time he did not know the nature of this phenomenon, it convinced him that he was dealing with minute parasites which live in or on the human red corpuscles like maggots within nuts. In fact, the black granules are merely the excrementitious matter produced by the parasites from the substance of the red cells and retained within their bodies to be ultimately released in the tissues of the host.

I must next explain some points for the lay reader. The blood of an average adult consists of some fifteen million million little discs of yellow jelly called the *red corpuscles*, floating in a fluid called the *serum*. About 3,400 red corpuscles touching each other and lined up in a straight line would occupy one inch. When a droplet of blood is taken from the finger of a patient and spread between two surfaces of glass under the microscope, these red corpuscles can easily be seen placed out flatly like plates on a table; and Laveran now discovered that in malaria many of them contain a little pale circular patch, without the yellow colour of the corpuscle. In fresh blood, however, these little patches can be seen to be constantly changing shape—that is, they possess *amœboid movement*; and they can also be seen to contain one or more minute black particles of melanin. They appear to grow in size within the corpuscle, and end by filling it up almost entirely, and then they contain a large number of melanin granules. When mature, they break up into separate portions called *spores* (ib. fig. 5); and then the corpuscle containing them breaks and the spores are liberated in the serum to infect fresh corpuscles (ib. fig. 6). These are the *asexual* forms of the malaria parasite —which was subsequently named *Plasmodium*.

But in addition to these forms Laveran saw what were evidently bodies of the same class but not contained at all in corpuscles—that is, bodies full of melanin lying by themselves in the serum (ib. figs. 11, 12). While watching one of these at Constantine in 1880 he saw, as I have said, the melanin-granules become violently agitated, and then observed that long filaments issued from the parent body and wriggled about violently as if attempting to escape from it (ib. fig. 19). He

PLATE III.

CHARLES LOUIS ALPHONSE LAVERAN.

thought that these filaments are *flagella*, like the flagella which enable many small organisms in water to move about; and the whole body with these so-called flagella attached became generally known under the name of the *flagellate body*. These bodies are now known to be, not really flagellate bodies at all, but the male *sexual* forms of the *Plasmodium*, the filaments being *sperms* (ib. fig. 21).

The whole history of the researches detailed in this book depends upon these sexual forms. Laveran was wrong in the rôle which he attributed to them; but that does not diminish the glory of his discovery, which has revolutionised medical science in the tropics. At that time, owing to the work of Pasteur, Koch, and others, many diseases were known to be due to *vegetable* parasites called *bacteria*, etc.; but Laveran's organism was evidently a minute *animal* parasite, belonging to the most elementary class of animals called the single-celled animals, or Protozoa.

Laveran next showed that his parasites occur in cases of malaria but not in healthy persons. He could not, however, work out their life-history completely; but in 1886 and subsequently C. Golgi, who was favourably situated in Pavia for the work, showed clearly that the asexual parasites reproduce themselves by forming spores (as mentioned above—ib. figs. 5, 6); that all these parasites in the body tend to produce the spores at about the same hour; and that it requires 72 hours for the quartan parasite to form its spores and 48 hours for the tertian parasites to do the same. He proved also that the attack of fever in the patient commences at the moment when the spores are liberated (just as Rasori had divined). This explains the difference between the quartan and the tertian fevers; and Golgi showed further that the shapes and appearances of the organisms which cause quartan (*Plasmodium malariæ*) and mild tertian fever (*Plasmodium vivax*) are different, so that the variety of parasite concerned can easily be detected at once under the microscope. A little later, Canalis, and Marchiafava and Celli, discovered similar facts regarding the malignant parasites (*Plasmodium falciparum*, Plate II), and showed that they differ from the quartan and mild tertian parasites; and Marchiafava and Bignami suggested that they are of two varieties, the malignant tertian and the quotidian. In 1885 Danilewsky discovered similar parasites in birds and several other animals; and subsequently Marchiafava, Celli, Bignami, Mannaberg, and others made many careful studies of the parasites, and of their effects in human

beings; Romanowsky found the proper way to stain them; and many observers verified these researches in various parts of the world, the literature amounting to some hundreds of publications. But up to 1894 no one had conceived the meaning of the sexual forms of the *Plasmodia* (ib. figs. 10, 11, 12). That is where our work commences.

The question now arose—an all-important question in connection with the prevention of the disease—How do these parasites manage to effect an entry into the blood of men and animals? Most observers, remembering that the disease often abounds in the neighbourhood of marshes, assumed at once that Laveran's parasites must be capable of living in a changed form in stagnant water; and some actually sought them there. Thus Grassi and Calandruccio suggested that a free living amœba is really the external stage of the organism. On the other hand, experiments to infect healthy persons by water from marshes, made by Marchiafava and Celli (1885), Marino (1890), and especially Agenore Zeri (1890) failed entirely.

But some other considerations should have been remembered by students of the subject, but were not. These minute animal parasites of the blood were quite possibly subject to a common law governing many larger parasites, called the law of Metaxeny or Change-of-Host. Until the beginning of the eighteenth century all parasites of men and animals were supposed, like other low forms of life, to be created in each host (that is, the animals containing the parasites) by "spontaneous generation." Very gradually the zoologists showed that these parasites, like other animals, develop solely from their own eggs. In 1790, however, Abildgaard made the remarkable discovery by experiment that a certain parasite not only develops from its own eggs, but migrates from one host, a fish, into another host, a water-fowl, which feeds on the said fish. The law was further developed in 1841 and finally established for certain parasites by F. Küchenmeister, physician to the Duke of Saxe Meiningen, in 1851–3. The famous German parasitologist, R. Leuckart, established many new cases. An important one was that of the *Filaria medinensis*, or Guinea-worm of man, which was found by Fedschenko sometime before 1869, at Leuckart's suggestion, to pass a stage of its life in the little "water-flea" called *Cyclops*. In 1877 Patrick Manson, in Amoy, China, showed that another *Filaria* of man, the *Filaria bancrofti*, or *nocturna*, has a similar development in a mosquito [7]. This was the first time that metaxeny was found to occur with any parasite between men and mos-

quitoes; but Manson did not observe the development to a stage much more advanced than that observed by Fedschenko in the *Cyclops* (page 127).

All these examples of metaxeny were concerned only with the higher parasites, the Helminths or Worms. But the objects discovered by Laveran belong to a much lower class of animal; and though many organisms of this class were later discovered in various animals, yet up to 1889 not one of them had been proved to be metaxenous. Then, however, Theobald Smith and F. L. Kilborne in America showed [12] that the little parasite of Texas Cattle Fever called *Piroplasma bigeminum* is communicated by a tick, though they did not find the parasite actually living in the tick, and therefore did not prove any alternate generation in them. Meantime, however, several diseases had been popularly connected with the bites of insects, etc. Many travellers related that natives ascribe a peculiar sickness to the bites of another kind of tick, and others said that the deadly nagana of cattle in parts of Africa is probably due to the bites of the tsetse fly—proved later by Bruce [18]; and it is now important to note that similar speculations connecting both yellow fever and malaria with mosquitoes had long been rife, though they were generally derided at the time and speedily forgotten.

I have already alluded to the curious utterances of Varro, Palladius, and Lancisi regarding insects and fever. Nuttall [51] gives statements by Lustig, Rübner, Koch, and myself to the effect that the peasantry in Italy, Tyrol, East Africa, and Assam seemed to have vague ideas of the same kind. Dr. R. H. Kennan informed me that he had found an old ordinance of Freetown, Sierra Leone, dated 1812, in which the inhabitants (mostly freed slaves) are enjoined to keep the road in front of their plots in good condition, in order to prevent the formation of "stagnant pools which generate disease and mosquitoes over the town." In 1848 Dr. Josiah Nott, of Mobile, Alabama, appears to have stated that both yellow fever and malaria may be transmitted by mosquitoes, and refers to the speculation as having been already advanced as regards malaria. In 1854, however, Louis Daniel Beauperthuy, a French medical man, born in Guadeloupe in 1808, gave the hypothesis in greater detail [5]. As a "travelling naturalist" for the Paris Museum in Venezuela, he studied both these diseases microscopically, and concluded that they are produced by a venomous fluid injected under the skin by mosquitoes, like the poison injected by snakes. Marshes, he

says, are dangerous because of the mosquitoes bred in them, not because of their effluvia. He surmised that several species of mosquito carry yellow fever, but mentioned especially " the *zancudo bobo* with legs striped with white "—probably the *Stegomyia calopus*,[1] which we now know is really the agent. But he gives no proofs in support of his opinions, which were only speculations based upon general thought and observation or fancy (page 424).

In 1881, and subsequently, Charles Finlay, of Havana, repeated a similar hypothesis, apparently independently of Nott and Beauperthuy [8]. His views, however, differ in an important particular. While Beauperthuy seemed to think that mosquitoes originally obtain the disease-giving poison from the marsh in which they breed (and apparently Lancisi believed the same), Finlay held (regarding yellow fever) that they obtain it from sick people. In other words, he thought that the insects simply convey it from patients to healthy persons. The proboscis of a mosquito which bites a patient becomes contaminated by germs in his blood ; the germs multiply in the proboscis, and then enter the blood of any person whom the mosquito bites next—just as bacteria may be carried on a soiled surgical instrument from one person to another. He thought that an insect which had only just bitten a patient could convey the virus ; but that the longer it lived after biting the patient the more the germs would multiply in its proboscis, and the larger the dose given to the healthy person would be. He also considered that a mosquito with striped legs (*Stegomyia calopus*) is the agent of yellow fever. He records some experiments ; but they must have been very doubtful, since we know now that mosquitoes which have bitten a patient must live for no less than twelve days before they can infect a healthy person. Like Beauperthuy, he rightly conjectured the species of mosquito which carried yellow fever, and actually placed them in the hands of the men who ultimately solved the problem. He was acquainted with Manson's researches on the development of *Filaria bancrofti* in mosquitoes (see also page 424).

In 1883 Dr. A. F. A. King wrote an able paper on the subject [9] in which he gives no less than nineteen reasons why mosquitoes are likely to carry paludism. These are : (1) That both paludism and mosquitoes are connected with marshes ; (2) that they both require a temperature of over 60° F. ; (3) are checked at freezing point ; (4) abound most near the equator and sea-

[1] J. Guiteras says *not* (*Lancet*, 18 June 1910).

coasts; (5) have an affinity for dense foliage; (6) can be screened off by trees; (7) can be transported by winds; (8) are encouraged by turning the soil; (9) are affected by "bodies of water"; (10) are diminished by cultivation and settlement; (11) keep near the surface of the ground; (12) abound most after sundown; (13) and in the open; (14) are destroyed by fires; (15) are not so common in cities; (16) are most prevalent in autumn; (17) are arrested by mosquito-nets; (18) affect infants (which are generally protected by nets) less than adults; and (19) attack whites more than other races. This was by far the best exposition yet given. Though arguments (4), (13), (14), (18), and (19) are not sound, while arguments (5), (6), and (7) are very doubtful, and though the most cogent argument of all was not known to him, the cumulative effect of his careful and well-arranged reasoning was very strong. Like Beauperthuy, he held that the insects bring the poison from the marsh and inoculate it by their bites. He was acquainted with Manson's work, but not with Laveran's. All these speculations were speedily lost sight of, and were not resuscitated until my researches had cleared up the question.

About the same time Laveran suggested the same idea [10, p. 457] in a short sentence: "Do mosquitoes play the same rôle in paludism as in filariasis?" he said. "The thing is not impossible, and we must note that mosquitoes abound in all marshy places."

Also about the same time, during his famous studies on cholera in India, R. Koch had the same notion; but he mentioned it only in his lectures to students [Nuttall, 51, p. 77, and Ross, 78, p. 73].

I now return to my personal narrative. Though it was certain that malarial fever is caused by parasites in the blood, and though the disease had actually been communicated from man to man by the inoculation of the blood of infected persons, still in 1894 we did not even guess, much less know for certain, how these parasites manage to pass from the sick to the healthy *under natural conditions*. This was evidently an all-important matter for tropical sanitation, because without such knowledge it was impossible for us to construct any scientific basis for the prevention of the disease, and therefore for saving a gigantic amount of misery in the world. It was my duty to attack the problem at this point. I call it here the Great Problem!

Though some *savants* had *suggested* that the mosquito carries the infection in some way, the world, even the medical world,

remained sublimely ignorant of and indifferent to the matter, and everyone held the old hypothesis mentioned on page 119 that, as its name implies, malaria is due to bad air from marshes. But I had long felt that this was not a sufficient explanation ; and in my voyage to Burma in 1889 (page 86) I saw cases which could scarcely be so explained, and began to consider the matter very carefully. At that time the medical services in India were so badly organised that medical libraries did not even exist in the principal stations,[1] and, while individual medical officers often did good clinical work, there was little investigation and almost no establishment for it, and we British cheerfully lived up to our reputation of being an unscientific if not an unintellectual race. Even the great bacteriological discoveries of Pasteur and Koch were scarcely recognised, or were ridiculed, and Laveran's was almost unheard of. Apart from individual workers such as Vandyke Carter, Lewis, and Cunningham, the services did not concern themselves much with medical investigation. Hence, when I began to think about the cause of malaria I could get neither advice nor literature, and at first fell into errors. To begin with, after my arrival in Bangalore I did some work (page 92) on the hypothesis that these fevers might really be due to some kind of intestinal poisoning, and published four papers on it [11]. I did not know that the French physician Broussais had anticipated me in this—and we were both wrong. In 1892, however, several writers in India began to publish papers on Laveran's parasite, but unfortunately described, not the *Plasmodium*, but a number of appearances in the blood which resemble it. The error was speedily detected by me and exposed in four more papers [13], and this, of course, led me (and others) to doubt whether Laveran's work was sound. By bad luck, though I searched innumerable specimens, I failed to find the *Plasmodium* at all, partly because my technique was too beautiful and complicated! I did not see Vandyke Carter's genuine work till I was in England in 1894. Thus all my laborious studies during these years 1890–4 were ineffective ; but the failures educated me and I had time to consider the subject very thoroughly and to become a competent microscopist. I had sent to England for Laveran's books in 1892, if I remember aright ; and I always subscribed to *The Indian Medical Gazette* and *The British Medical Journal*. Laveran's descriptions and drawings of the parasites were, as everyone admits, so poor that it was not surprising that few recognised

[1] I understand that this has not even yet been always remedied.

his bodies at first from his writings. At any rate, I failed to do so, and therefore thought that his work was unsound, or even a pretence.

On my arrival in England, in March 1894, I went at once to Professor Kanthack, the pathologist, at Bartholomew's Hospital, who assured me that Laveran's discovery was sound, and referred me to Dr. Patrick Manson, then living in Queen Anne Street, Cavendish Square. I wrote to him at once, and called on him on about 10 April 1894. Within a few minutes he showed me the Laveran's bodies which are technically called " crescents," in a stained specimen of malaria-blood, and I recognised at once that no such bodies exist in healthy blood. My doubts were now removed ; and in a few days Manson demonstrated the other forms of the organisms to me in a patient lying at Charing Cross Hospital, and also took me on several occasions, with great kindness, to the Seamen's Hospital (at the Royal Albert Dock, in East London) and made me acquainted with translations of the illuminating monographs on malaria by Mannaberg and by Marchiafava and Bignami in Vol. CL of the New Sydenham Society's publications. Subsequently the distinguished microscopist, Dr. H. G. Plimmer, F.R.S., told me that he had similarly demonstrated the bodies to Manson some time previously.

Dr. Manson (afterwards Sir Patrick Manson, G.C.M.G., F.R.S., M.D.) was then a good-looking, rather tall man of fifty, who had practised for some years in China, where he had done excellent medical and parasitological work on various skin-diseases and also on the terrible deforming malady called elephantiasis. He had practically shown this to be due to the small worms which live in the lymphatics and the blood-vessels and are called *Filaria bancrofti*. But, more than this, either on his own initiative or on that of Bancroft, he had shown that the young of the parasites are sucked up with the blood of patients by mosquitoes (species then undetermined), in which insects they grow and develop to a considerable size [7]. He did not work out the whole life-history of the *Filariæ*, but thought that the insect dies after laying her eggs on water, when the parasites escape out of her dead body into the said water, in which they are drunk up by healthy persons, who thus become infected. This life-history, which he discovered in 1877, was very like that of *Filaria medinensis* in *Cyclops*, discovered previously by the Russian naturalist Fedschenko, on Leuckart's suggestion, after 1858. Both life-histories were incomplete, and Manson's conjecture that the

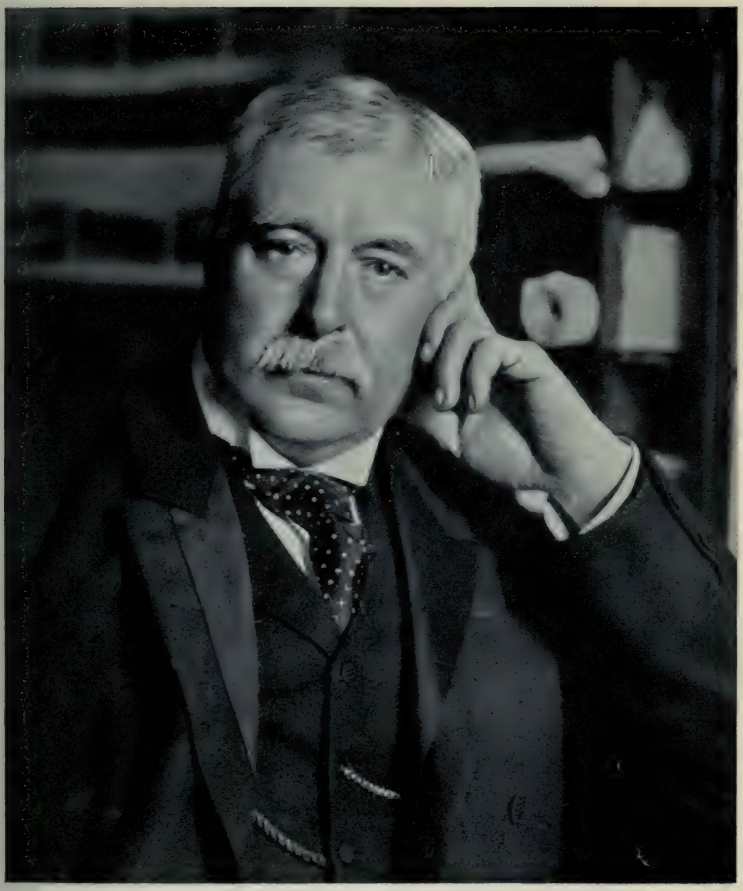
filaria embryos return to water after living in the mosquito was wrong ; for after my researches had shown that mosquitoes not only take the malaria parasite from infected persons but also inoculate them after a week or more into healthy persons (1898), James, Low, and others proved a similar procedure regarding *Filaria bancrofti*. I gather that Manson could scarcely have done much actual *experimental* work on the subject, because he certainly instructed me that the insects live only from three to five days after feeding,[1] while the *Filariæ* are now known to require three weeks for development in mosquitoes. I gathered also from him that he had worked only with the insects caught in the go-down in which his servant infected with *Filariæ* lived (see also Manson's article in Andrew Davidson's *Hygiene and Diseases of Warm Climates*, 1893, p. 771, Edinburgh, Young J. Pentland). But undoubtedly he observed the first stages of the parasite in his mosquitoes (probably a *Culex*) and figures them correctly ; and the above facts do not detract from the very great merit of his work.

Manson had now retired from China since some years and had commenced practice in London. He treated me with the greatest kindness, and I visited him often and learnt all he had to tell me. I do not remember having done malaria work in London during the summer of 1894 (and was in Switzerland for six weeks) ; but in November I called upon him again and found him just starting for the Seamen's Hospital. I went with him, and remember distinctly that as we were walking along Oxford Street at about 2.30 p.m. he said to me : " Do you know, I have formed the theory that mosquitoes carry malaria just as they carry *Filariæ*." I replied at once that I had seen the same conjecture in one of Laveran's books [10].

Manson then explained his cogent reasons for his belief. As I have said (page 123) others had previously suspected that malaria is carried by mosquitoes, but mostly on very inadequate grounds ; and we did not even know of these writings at that time. Manson now added a new and stronger argument to those already mentioned. We knew the function of the *asexual* forms of the parasites, but were ignorant of the functions of what were later proved to be the *sexual* forms—the so-called crescents and flagellate bodies (Plate II, figs 11–20). Shortly after blood containing these forms is drawn from a patient's finger the crescents swell up, become oval, and then spherical, and lastly sometimes emit the long and actively motile fila-

[1] I showed in 1897 that mosquitoes, even in captivity, live much longer than five days (page 229), merely if they are fed frequently enough.

PLATE IV.



PATRICK MANSON.

ments (ib. fig. 19) which were then wrongly called flagella, and these may sometimes be seen under the microscope to break away from the parent cell and to wriggle about rapidly in the fluid of the blood (ib. fig. 21). They had long been subjects of discussion. Some observers, chiefly of the Italian school, held that they represent only the dying struggles of the parasite; others, including Laveran and Danilewsky, thought that they are really living bodies, and Mannaberg even suggested that they may be connected with the life-history of the parasites outside the human body—though he did not explain how this could be. Manson now offered an explanation. He thought that when the mosquito sucks the blood, these sexual forms enter its stomach, and there, in a few minutes, emit their motile filaments just as they do under the microscope. The motile filaments, he thought, were *flagellated spores* which next pass through the walls of the mosquito's stomach and take up their abode in its tissues, where they must develop further into some unknown stage and then enter drinking water. Here he was wrong.

But I was immensely impressed with this argument and determined to test the hypothesis thoroughly on my return to India. A few weeks later Manson published a short article describing his hypothesis [14]. Of course we discussed the best procedure for me to follow and decided that I should work if possible with crescents (now known to be the sexual forms of *Plasmodium falciparum*). Manson described his technique with *Filaria bancrofti*; but, as this was a very much larger parasite, his experiences with it did not help me much with malaria. I tried to obtain works on mosquitoes, but failed entirely—probably because, very unwisely, I did not inquire at the British Museum; a most unfortunate failure, because there *were* some entomological papers on the subject; and I never even received Ficalbi's treatise which appeared next year [24]. I was also ignorant of the Romanowsky method of staining the malaria parasites, though this had been discovered some years previously. In fact, I went forth very badly equipped for the fray.

Before leaving England in March 1895 I won (20 March) the Parkes Memorial (triennial) Prize offered at Netley Hospital to members of the medical services for the best essay on malaria [16]. It consisted of a gold medal and seventy-five guineas. I have just read, for the first time since it was written, my draft of the essay. It admits Laveran's parasite and Manson s induction, describes (with many microphotographs)

the natural appearances in the blood which were being mistaken for *Plasmodia*, shows why I had failed to find the latter, and deals with the numerous fevers which were then confused with malaria. But the chief merit of the essay lay in its destructive analysis of textbook theories of aerial convection of the infection from marshes or the soil. I concluded that if such convection exists it ought to diminish, like light or gravity, as the square of the distance from the source; so that, for instance, "a square yard of (boggy) soil at the distance of one yard will infect as much, *ceteris paribus*, as a square mile of soil at the distance of one mile will." It was a logical critique of these old medical dogmas. Thus, in discussing the alleged effects of "climate" on malaria I remark:

"We see fevers of apparently similar nature in every part of India, no matter how diverse the climate. Speaking from my own experience, I have seen the same or similar fevers in the jungles of Burma, full of decaying vegetation and drenched with heavy falls of rain; in the Andaman and Nicobar islands, covered with jungle, it is true, but where one would think the air would be purified by the incessant and strong sea-breezes; on the flat coast of Madras with its rice-fields and general irrigation; on the table-land of Bangalore with its bare rocks, scanty soil, rock-bound tanks, absence of luxuriant vegetation, cool and equable climate and red soil; on the similar black-soil table-land of Secunderabad with a ground which seems to dry in a few minutes after the heaviest fall of rain; in the desert of Sindh and the Bolán Pass where there is scarcely a single tree to be seen, where the rain seldom or never falls, and the air seems to contain not a particle of moisture; and, lastly, at Quetta, 5,500 feet above the sea, a flat plain surrounded by barren mountains, scantily supplied with water by an open running stream. What inferences are to be drawn respecting the effect of climate and so on from such an experience? One remains simply in amazement before the facts. How account for the malaria on the paludal hypothesis in the desert of Sindh? How account for it on the effluvium theory amongst the sea-breezes of the Andamans? How explain its prevalence *everywhere*, in spite of such difference of climate, soil, vegetation, and moisture?" We can now explain these things. I finished the essay with the words: "In conclusion, I venture to say that we may shortly expect the discovery of certain facts in

connection with Laveran's parasite which will reform our views concerning malaria."

Prophetic, but rash! My motto was *Da nobis lucem, Domine.* The essay is full of considerable reading (at the British Museum Library) and how it could have been completed while I was writing two novels at the same time is a mystery to me now!

My work on natural objects found in the blood was not quite worthless, either. I invented several beautiful new methods, and at one time held that the "blood-plates" may quite possibly be spores of certain kinds of leucocytes! But I have never touched this work again [85].

Before returning to India I invented a small portable microscope, useful for the high powers but capable of being slung round the shoulder like a pair of binoculars. Mr. C. Baker of Holborn brought it out for me and sold many of the instruments.

During our talks, Manson and I frequently discussed the possibility of his doing the required researches at his hospital at the Royal Albert Docks near London, and I remember urging (what was really self-evident) that, if sufficient human cases were not available, he could work with birds' malaria and with English midges and gnats—neither of us seemed to know that mosquitoes *are* gnats! I remember also suggesting the use of incubators; but, for reasons which I have forgotten, he was unwilling, though I urged such researches again (page 147); and he certainly did not do the work.

It has also been said that he "selected" me to verify his theory. Quite untrue. I selected myself; and no one else really touched the work till I had done it.

Really the only point of exigence in Manson's hypothesis was the single fact that the process of "ex-flagellation" (page 128) never occurs until about ten minutes after the blood is drawn (and kept liquid)—thus suggesting that it is meant to occur in the stomach of *some suctorial insect.* But even this was not *convincing* till Sakharoff and I proved that the process was not merely a death-struggle (pages 194, 195); and all Manson's secondary speculations based upon it turned out wrong. Yet some people write as if his hypothesis itself sufficed to prove the mosquito-theory! It was not a mathematical induction like that of Leverrier and Adams regarding Neptune, but only a strong biological working hypothesis.

CHAPTER IX

FIRST EXPERIMENTS. SECUNDERABAD, 1895

On 28 March 1895 I embarked at London on the Peninsular and Oriental Steamship *Ballaarat* for India; and now commenced the four eventful years of severe labour which solved the malaria problem. The details of the work are recorded minutely: (1) in my two manuscript volumes of daily laboratory notes; (2) in series of frequent letters to my wife and to Dr. Manson, and in various other letters; and (3) in numerous publications. I wish, above all, as one motive for this book, to place the reader exactly in my position, even though he be entirely a layman in these matters. He shall (if I can help him to it) look through my microscope, scribble my notes and letters, and post about over India with me! For this purpose the narrative shall consist chiefly of extracts from letters and notes written in the heat of action. If I fail I can only plead my best endeavours; if I succeed he will at least understand something of the passion of investigation!

The ship was a slow one, which touched at Gibraltar, Malta, and Brindisi, and I commenced at once, as I wrote to my wife, doing microscope work and pricking people's fingers, much to their amusement; and I hoped soon to get hold of cases of fever. On 6 April I wrote to her: "We arrived at Malta at 7 a.m. yesterday and did not leave till 3 p.m. I rushed off to the Civil Hospital at once, and was directed to go to the Naval Hospital, which is right across the Grand Harbour. I went there and examined the blood of three Malta-fever cases. None of them were malaria cases. I found I had gone to the wrong Hospital, as there was a man called Hughes who had specially studied Malta-fever. . . . Just as it was time to be leaving I saw Hughes, who turns out to be one of the competitors for the Parkes' Prize [which I had just won], a very nice and clever man. He gave me a French tract written by him on Malta-fever, and we are going to write to each other. I left then, and was only just in time for the steamer, but did some useful work in the little time at my disposal."

This Malta-fever (or Undulant Fever) is a most prolonged and trying kind of fever which haunts parts of the Mediterranean littoral (and other countries), and was shown by (Sir) David Bruce in 1886 to be due to a minute vegetable organism. In 1895 Captain Hughes, of the Army Medical Service, was studying it further in Malta, and in 1897 he published an excellent book upon it. At the time of my visit he was glad to have corroboration from me that the malaria parasites were not present in the cases. He was a fine young officer, who was killed a few years later in the South African War. Subsequently the commission, of which Bruce was the head, proved that the fever is spread by means of goats' milk—altogether a great piece of work.

We proceeded, and I wrote: "We have had a delicious voyage along the coast of Italy to Brindisi. On our left we have been passing villages, castles, and ruins, and on our right, far away, there have been the high, snow-covered mountains of Greece. Think of that!" This was the first time I had seen Greece. On April 8, on leaving Brindisi, we had a closer view. "I should like you to have seen the Isles of Greece. We passed right among them. The captain went a little out of the way (?) in order to run between the islands of Cephalonia and Ithaca (where Ulysses and Penelope lived)! We saw the Gulf of Corinth and Sparta [Peloponnesus] with the mountains covered with snow. . . . I even believe that we saw Mount Parnassus, where Apollo and the Muses lived. I would not have missed the sight for a great deal, but hope that we shall yet be able to see it together some day." It was indeed a glorious evening, that of 8 April, as we steamed across the Gulf of Corinth and watched the rays of the setting sun pointing out one by one those sublime summits—which we identified from the captain's charts—the birthplace of Europe, the very sight of which fills one with ambition! We did indeed visit the spot together in 1896; and I saw it again in 1913, and was torpedoed close by in 1917.

I continued in the same letter (10 April): "I have been working steadily with the microscope, dissecting cockroaches (because they have a parasite something like the malaria parasite), and also preparing experiments for my big piece of work. I see clearly now how important the work in connection with the mosquito is. . . . But then *does* it (the malaria parasite) occur in the mosquito? If I find it by the end of the year I shall certainly try to get three months' privilege leave and come home to show my specimens and to bring

you out." On the 15 April, in the Red Sea, I wrote: "I have been working away at the microscope and spent yesterday looking for parasites in a small flying-fish; I found several very interesting ones. Also I have been reading a lot of medical work in preparation for the mosquito work; but I have failed in finding a single case of malaria on board, which is a nuisance."

At Aden we transhipped into the *Thames* for Bombay, which we reached on Sunday, 21 April. I was obliged to waste two days here before proceeding to Secunderabad, to which station my regiment, the 19th Madras Infantry, had been moved from Berhampur; and I spent the time in looking for malaria cases at the Civil Hospital. On the 23rd I left for Secunderabad, which we reached at midnight, 24–5th, and I lived there in a bungalow *en garçon*, with Captain Thomas, the Adjutant, and Lieut. Hole, both first-rate fellows. We had our mess, and there was the Secunderabad Club, where we played golf and tennis; but I kept no ponies, as I expected to be put upon special malaria work at any time. It was at the height of the hot weather; the ground was a desert and the wind hot and dry; but after paying the usual ceremonial calls, I got to work at once in the best of health and spirits. Probably my first letter to Manson has been lost, but progress is reported in those given below.

First it must be explained (for scientific men) that although fifteen years had elapsed since the *Plasmodia* had been discovered by Laveran, and nearly ten years since the structure and habits of the different species in the human body had been elucidated by Golgi, the only person who had yet published a paper on them throughout the great Indian Empire was that pioneer, Professor H. Vandyke Carter, Principal of Grant Medical College, Bombay (*Scientific Memoirs of Medical Officers of the Army of India*, 1887), and he had found them only in nine out of seventy-three cases clinically diagnosed as malaria. Perhaps other workers had really found them (such as Dr. Crombie) without investigating them; but there was much doubt on the point owing to the frequency with which artifacts were then being mistaken for them (page 126). It is amazing that the two official investigators specially employed for years by the Government of India had not observed the parasites (page 263). Carter's admirable paper was published before he knew of the illuminating work of Golgi, which had then only just been published; and I was therefore obliged to spend some time in confirming that work for India, and also in

looking for possible new Indian species of *Plasmodia*. Moreover, in 1895, our modern methods of staining specimens had not been fully developed, and I therefore worked with *fresh* blood and mosquitoes' tissues, and it was lucky that I did so. My principal object was to study the *Plasmodia*, not in man, but in the mosquito—a thing which had never been yet attempted. Manson's labours on the *Filariæ* in mosquitoes were not much help, because these are large parasites of quite a different class to the *Plasmodia*; and I had to find out most of the technique for myself. There was nothing to guide me as to which exact species of mosquito was concerned in malaria; and I had not even been able to obtain any scientific literature on Indian mosquitoes, or even on the classification of mosquitoes (*Culicidæ*) in general. It was necessary to investigate everything *de novo*. First, I required cases of *Plasmodium falciparum*, with numerous sexual forms (Plate II, figs. 11–20) (crescent-spheres, spherules, page 138) in their blood; secondly, numbers of mosquitoes bred from the larva; and, thirdly, I had to persuade the mosquitoes to bite the patients.

The letters were written *currente calamo*, generally after a hard day's work and in time to catch the mail. They are here transcribed exactly from the originals, bad grammar and mistakes included; but I have added some elucidations in square brackets and in notes, and present my apologies to the reader for my style. Altogether, from April 1895 to February 1899, I wrote 110 letters to Manson, containing almost exactly 1,000 words each—or about one word to every ten people killed by malaria in India alone *every year*—perhaps worth the trouble! Apparently the only one of the letters which has been lost was the first, written in April 1894, from Malta or Brindisi. We begin therefore with the second; and I copy it and the three following ones almost entire because I believe that medical readers will like to see them.

SECUNDERABAD, DECCAN,
1 *May* 1895.

DEAR DR. MANSON,

I write to report progress, and hope you will excuse size of paper. After leaving Malta I examined the blood of all likely cases on board ship (and none were anything but remotely likely) without any result. The day after reaching Bombay I went to the Civil Hospital and immediately found a

woman with tertian from Ahmedabad, with splenitis but not treated with quinine. The parasites were rare but I found one in the marginal clot in five minutes. There were only about six in the first specimen, crescents and mature spheroids very deeply pigmented. The same evening and the next morning I found the same forms apparently, and in same state, but had not time to work properly as I had to get luggage through the customs. I have been here now less than a week and though I have been very much occupied in calls and business have spent four days and more with the microscope and out of 11 cases have found 5 due to the parasite. Carter found 9 only during some months. The heat is awful and the wind like the blast of an engine-furnace. There were only convalescent cases drenched with quinine in my hospital, so I had to drive three miles to the civil hospital in the heat of the day. There were five cases in one ward, in three of which, though they all had had quinine, I found the parasite in 30 seconds (2nd & 3rd fields) each—crescents and large spheroids deeply pigmented. The two cases in which I did not find it were convalescent and were discharged next day. I immediately made a push for the mosquito. By bad luck hardly one was to be found, the air being *fiery* and the three I caught and put into bottles were too frightened or otherwise unwilling to bite ; I had no mosquito net.[1] Up to date I have failed in getting a single mosquito to lay hold, which I think is because I have had to catch them. I am now breeding numbers and have a net ready for use. The three cases were examined carefully the whole day the next day and I found one of the reasons why I could not find the parasites before. Above 90° phagocytosis is so quick that all free forms are gobbled up in *twenty minutes*. I used to take my specimens home generally. I found two beautiful forms ready for flagella [Plate II, fig. 17] and had logged their position and moved off to study some crescents ; on coming back 10 *minutes* after making the specimen they were both inside phagocytes ! Next day in my hospital I found two cases of intraglobular parasites with very fine pigment. Malaria was diagnosed in both cases

[1] I used to put the patient lying down on a bed within a bed-net and release the mosquitoes at him inside. Whether I devised this method or Manson had used it previously I cannot remember (see page 128).

within one minute each. I am sorry that I foolishly packed your chart in the wrong box, now coming by goods train; so that I cannot quote numbers of forms. Four cases in my hospital have hitherto given negative results; but I am at present engaged in looking at the parasite rather than for it. I am delighted and surprised at numbers of cases and ease with which they have been found. My microscope is invaluable. Have you read Crombie's speech in Indian Medical Gazette for March? He (and all the doctors in the country) are furious with Mr. Hart's speech. He is trying to throw cold water on microscopic diagnosis of malaria and says that he found the parasites in only 4 cases out of 50 in Calcutta in January. My results within a week and before the fever season give 50%. He talks too of half an hour being required, as if that was the average time—so I am keeping careful notes of time taken for each diagnosis. When present, the parasites generally swarm—1 or 2 in three fields about. To-day, however, the two cases in my hospital show not a trace of any, but I am off to them again after sundown. Both are fresh cases with very low fever and much headache. Evidently the laws of these sudden appearances and disappearances must be made out if possible. All the cases are beautiful; and I am wild with excitement; but these mosquitoes! They *will not bite*. I tried them on myself for experiment, but having been caught they are frightened I suppose. I expect a crop of new ones in one of my bottles this evening and hope a fever case will show itself to suit. No flagella been seen as yet; but I certainly saw a spent crescent in one specimen. I believe (or rather think) from what Carter says that they are released almost immediately in a high temperature—but we shall see. The finely pigmented bodies seem to belong to quite a different species from the others. I am touting round the country for cases. The doctor in charge of the policemen refused to transfer them to me for experiment and there are difficulties in the way of my experimenting at his hospital. By bad luck I am engaged for tomorrow.

2 May. All my mosquito grubs have been killed owing to my foolishly putting the bottles in the sun. I have consequently started a new lot; but it is vexatious. . . . Unfortunately the principal medical officer here seems to be putting

me on every duty conceivable; but I shall go and look for cases at the civil hospital again tomorrow. . . .

Yours truly,

RONALD ROSS.

The 19th Madras Infantry was then stationed at a cantonment of Secunderabad called Begumpett, situated close to the outflow of the great tank of Secunderabad. The hospital and the hutments of the men and their families were placed not far from the tank, but the officers' mess and quarters were farther away, and our bungalow was about a quarter of a mile from the hospital. The hospital was a one-storied building, containing, I think, two wards, besides offices, and it was here that I did most of my important work. Of course this work was carried out in addition to my duties as regimental surgeon, and I had to pay all the necessary expenses in connection with it myself. Crescents, spheroids, flagellates, flagellate bodies, were the names which I used for denoting the various stages of the sexual forms of *Plasmodium falciparum*, the so-called malignant parasite (Plate II, figs. 11–20). The microscope to which I refer was the one recently invented by myself called the Diagnostic Microscope (page 131); and the chart referred to was one recently invented by Manson, who had cut out a large number of coloured drawings of malaria parasites found in various books, especially in the Italian ones, and had stuck these drawings on a single sheet and had then caused printed copies to be made of the whole lot. The different forms were numbered, and he proposed that these numbers should be used by observers to record the forms found by them —a very convenient invention. A little before this date Mr. Ernest Hart, Secretary of the British Medical Association, had visited India and had criticised the medical services somewhat strongly, and, as will be seen by the letter, some of the doctors were rather offended. The layman should know what phagocytes are. They are certain of the so-called white cells of the blood—little independent living masses of protoplasm with nuclei, which surround and ingest extraneous bodies such as particles, bacteria, or malaria parasites which have effected an entry into the blood—the process of phagocytosis being easy to watch under the microscope in the case of the malaria parasites, and being a very interesting phenomenon. Indeed phagocytes have often been considered to be the policemen of the blood—with this difference, that the phagocyte, instead of arresting the culprit, swallows him on the spot; at least he

is the scavenger of the blood. Of course, when a patient has been taking enough quinine for some days, his *Plasmodia* become so much diminished in number that few or none can generally be found in the small drop of blood taken for a single examination, say one cubic millimetre—that is, one three-millionth part of the total amount of blood in an ordinary man.

SECUNDERABAD, 15 *May* 1895.

DEAR DR. MANSON,

The fortnight since I wrote to you has been made up partly by disasters and at last by success. I had a bad case requiring constant attendance; the two ague cases in my hospital ceased to show parasites; when, with much difficulty I had got two cases out of the bazaar, all my mosquitoes died again; and next day when I had got a fresh crop of mosquitoes my two cases ran away, because I pricked their fingers, in spite of my giving them a rupee a prick! I now saw that it was necessary to get a Sepoy case and so spent a day at another regimental hospital. I examined a number of cases in vain (all being soaked with quinine); but at last cast my eye on a case just come in for dysentery: success—two crescents in specimen—case transferred to my hospital. Next day there were a fair number of crescents (11 May) and nothing else—just the sort of case required—but the mosquitoes would not bite. The temperature was between 90 and 100° and as dry as a furnace; so that the mosquitoes died shortly after being released from the bottle. Nothing I could do persuaded them to bite. The same happened on the 12th, the crescents *swarming*, three or four in a field, the mosquitoes as obstinate as mules and myself nearly frenzied. I watched specimens however and saw that at this temperature the crescents show but little change, only a very few becoming spherical. I did not see a single flagellate body though I must have looked at hundreds of parasites on 11th and 12th; and I saw only a few doubtful spent crescents.

Next day (13 May, my birthday) there was better luck. It had rained at night and the morning 6.30 a.m. was cool—I obtained four full mosquitoes. I also found out how to make the insects bite; that is by wetting the bed and mosquito-net with water—this makes them hungry in a moment. The

finger blood was swarming with crescents and crescents only. By an accident I did not know that a mosquito had bitten till an hour after he had done so. I bottled him, smoked him (or rather *her*), and separated the abdomen just as you had described; but it was now so hot and dry again and the blood dried almost as soon as squeezed out so that I had to moisten with normal salt solution. Results at once :

1. Parasites present. This was interesting as owing to exceedingly pointed ends of crescents [drawing] I thought they might stick in proboscis.

2. Numbers apparently same as in finger blood (very numerous).

3. Forms, *apparently* nearly all spherules, many spent crescents (?) and very few distinct crescents.

With regard to (3), however, I saw that there were many elements of doubt, requiring experiment. I examined the other three mosquitoes within five hours with identical results in all ; and then set to work to experiment. First, the salt solution which it was necessary to add in all the cases, does, I find, turn a large number of crescents into spherules in a minute at this temperature (I believe Laveran says so, but have not time to look him up). In the spherules produced in this manner the pigment granules *swarm* of course for a time ; but only once did I observe (in salt solution) a flagellate form, and that was unsatisfactory. This showed, however, that the spherules in the mosquito might be due only to the salt solution. Moreover, I noticed that in pure blood, in the marginal clot, the crescents change much more frequently than elsewhere ; so I thought that the change in the mosquito might be due to the clotting of the blood (as is likely). As for the *spent* crescents, they seemed very numerous, but I was not certain that they *were* spent-crescents. The three forms are [drawings]. In the obscurity of the exuded hæmoglobin. I could never make out satisfactorily whether the [drawing] spent-looking form had not really a cell wall round it. I noticed no flagellate bodies.

Next day 12th I spent over six more mosquitoes ; much hindered by the rapid drying ; trying water, glycerine, Canada balsam, salt solution, etc. In none of the mosquitoes could I get the specimen in the same condition as my control specimens

from the finger; and I remained unsatisfied, both about spherules and spent-crescents; though I was forced to confess that these *appeared* much *more* numerous and the crescents much *less* numerous than in my control specimens. To-day I have been at work on three mosquitoes. One had no parasites, by which I could see that he had got into the net after biting someone else outside (the moisture of the net is attracting outside gentry). The other gave no more certain results, though I got him twenty minutes after biting and examined the blood pure. I have been trying control specimens on ice, too, and found one good flagellate form with *very* delicate flagella, scarcely visible, nothing like the thick flagella I saw with you. I find both spherules and spent-crescents in different conditions of the same control specimen; which is irritating because I cannot satisfy myself as to cause. At last, this evening, however, my dull brain suggested the proper course—to try dry *smears*. I got a half-hour mosquito and smeared his blood with the needle on cover glass, inverted the same after drying in air and simply laid on slide, using oil immersion but no medium. *Nothing but spherules*. Not a single crescent and only one or two masses of spent pigment (?). A control finger blood made in same way, gave, of course, *all* crescents and no spherules; while a finger blood, which had been kept for two hours and was then opened and dried, gave *mostly crescents* and a few *spherules*.

This last experiment then seems to me to *prove* that the crescents undergo change into spherules in the stomach of the mosquito as outside; and to *suggest* that they do so faster than in control specimens. I must satisfy myself much more however, before I state so much definitely. I am, as it were, touching the matter with my soul before my intellect has reached it; just as a phagocyte touches a microbe with its bioplasm while the nucleus lumbers up behind (!). Talk of phagocytosis! I have seen things these last three days; but have no time to write about them. I am dead beat now and the lids of my right eye are swollen and painful—at work from 7 a.m. to 7.30 p.m. with snatches at breakfast and tea.

16 May. Four more hours work but no good results and have to break off to write mail letters. Certainly crescents are very rare in mosquitoe's stomach after half an hour, but

remain in numbers up to 12 hours in finger-blood. Have tried a thick film (to avoid pressure) kept for 2 hours and then smeared and partly stained [finger blood]. Numbers of crescents, many spherules and some clusters of pigment. I hope by degrees to improve my manipulation and get a clear certainty whether crescents do or do not transform more quickly in mosquitoes than in specimens.

I mentioned that two men in my hospital had different looking parasites to other cases. These on reference to Marchiafava turn out to be exactly like his tertian, while the others are quartan seemingly; but I must work it out when I have time. I sent your charts to Government the other day, with letter asking them to buy for hospitals.

A few days ago I saw Lawrie who read me part of a letter from Hehir, in which he said he found 30% of the corpuscles infected in a case of malaria. One third! . . . To close, I want to try an experiment—keep an infected mosquito for a week in a bottle containing water, and then drink mosquito and water together. But it ought to be tried systematically. I shall get a number of mosquitoes in bottles tomorrow. Case still swarming with crescents—no fever.

Believe me,
Yours truly,
R. Ross.

SECUNDERABAD, 22 *May* 1895.

DEAR DR. MANSON,

On the 17th (the day after I finished my last letter) I made another dry smear of mosquito blood, which, like the former one showed spherules and pigment only. On staining it with water solution of methylene blue without fixing, the cellular structure of the spherules was shown more clearly; and they looked very different to similarly stained specimens of (*a*) fresh blood and (*b*) blood that had been kept liquid for two hours and then smeared and dried; the former of which gave, as before, only crescents, and the latter mostly crescents and a few spherules and pigment masses.

This confirmed me in the view that crescents are transformed much more certainly into spheres in the mosquito's stomach than in specimens. I now determined however to make another attempt at liquid specimens, because I hate dry

smears; so I thought it out, and gave the patient instructions to let the mosquitoes fill themselves completely, while I provided myself with a small pair of tweezers with which I could squeeze out the mosquito's stomach with one hand, while I held the cover glass with the other, ready to clap on. The result was my first perfect specimen of mosquito blood. About twenty minutes (?) had elapsed since the mosquito had begun sucking; and the hæmoglobin was of course much diffused. The blood swarmed with perfect spherules, not very large (about 6μ in diameter) but showing an almost eruptive movement of the pigment as if flagella were about to burst forth in a moment. I looked in vain for flagellated organisms; not one was to be seen, to my surprise. In a very few minutes the pigment-oscillation (the intensity of which was much greater than in the control specimens) began to quiet down. Many of the spheres were *shaking*, but, as I have said, not one was flagellate. The general view however was very different from an ordinary specimen. All the spheres had *perfect* circular contours and I now noticed that they seemed to be in much larger numbers than in finger blood (contrary to my first impressions). Here are my notes in thin fields mostly.

7. 3. 9. 7. 2. 2. 3. 2. 2. 0. 2. 2. 1. The fields were consecutive ones, and the numbers will give you some idea of the quantity of parasites. I made a specimen of a second mosquito at $\frac{3}{4}$ hours after beginning to suck and the results were precisely the same.

On counting the numbers of crescents, spheres, and masses of pigment respectively, I obtained

	Crescents.	Spheres.	Pigment.
1st specimen	7	80	8
2nd ,, ...	14	30	9
2nd. spec. next day after 19 hours . . .	0	8	32 (?)

In both these the number of crescents is a great deal too favourable because it was possible to examine field after field without finding one. Of course the blood last sucked by the mosquito would contain most crescents.

Next day (18th) I began with a finger-blood specimen, which gave when fresh

Crescents and ovals.	Spheres.	Pigment masses.
39	10	1

Then I made two more liquid mosquito specimens at about ½ an hour, my 18th and 19th mosquitoes.

Up to date I had observed only two flagellate bodies, both in finger blood. I was astonished I did not meet more. Always the spheres seemed ripe for flagella, but they did not come (temperature of air 95° F.). Now, directly I looked at mosquito 18 I saw flagellate organisms in every field. I saw nine altogether in a few minutes, but instead of counting them I wasted my time looking at them. Two fields held two each and I could have found numbers, I do not doubt, if I had looked at once over the specimen. As it was the spectacle afforded by mosquito 18 was amazing. All the spherules seemed bursting with excitement and exceedingly numerous. In contiguous fields I noted,

$$6.\ 10.\ 8.\ 7.\ 10.\ 14.\ 12.\ 16 = \frac{83}{8} = 10{\cdot}0$$

But when I came to examine special fields I found,

Sixty parasites in one field,

Seventy-two in the next! all swarming and ready to burst. I think you will confess that no one has had such an experience as this in finger-blood. On looking through mosquito 19 I found the same swarms of spheres, but no flagellate organisms. I lit a cheroot, went home (though it was only 12 noon) and slept for three hours; the spectacle of flagella had ceased after about 10 minutes.

Next day (19th) I began with mosquito 20 of yesterday. Mostly pigment masses but about 20% still cellular. At 11.50 I made specimen of a fresh mosquito (21) at ½ an hour, with a presentiment of what I was going to see.

4 flagellates in 1st field of 20 spheres
5 „ in a field of 12 spheres
3 „ in a field of 10 spheres.

Flagellate organisms everywhere; one or two in every field. I must have seen 30 or 40 though I examined only a few fields, being hardly able to tear myself away from individual organisms. The flagella were excessively fine, quite invisible in open spaces and seen in hæmoglobinous serum and clot; but the flagellate organisms were unmistakable because they were being dragged about all over the field generally, or were quivering and being

shaken as a dog shakes a rat. The manifestation nearly ceased in a few minutes, though I saw flagellates for ½ an hour afterwards. Mosquitoes 22 and 23 at 70 and 90 minutes gave no flagellates and but few parasites comparatively. The patient was put under the net again. Mosquito 24 gave results at ½ an hour as mosquito 21 ; 5 flagellates in one field of 12 parasites. Flagellate organisms everywhere, especially in open parts (?). Manifestation lasted in its intensity only for about five minutes.

On 20th and today I did not observe anything new, and I did not work yesterday. . . .

23 May. Another disaster among my mosquito grubs owing to the heat I suppose. All dead, but a crop of small mosquitoes which seem *all to be males* and won't bite !

As mail goes today I will close with summary of results. I am inclined on the whole, after examining 28 mosquitoes, to gather that the stomach of the mosquito is very probably the natural *locus* for the crescent-spherule-flagella metamorphosis [Plate II, figs. 13, 15, 17, 19] ; and I base my opinion on the following five reasons.

1. The number of parasites in mosquitoes appears to be greater than in finger-blood (especially in some mosquitoes).[1] In mosquito 18 I found fields of 60 and 72, as I have said ; but in a control of the same time the largest number found in a field was 14. Further study is required.

2. It is certain (and very important) that in mosquitoes, at half an hour after beginning to suck, practically all the crescents have turned into perfect spheres ; whereas in control specimens which have been kept for half an hour and much longer 70% of the parasites remain crescents and ovals (90–100° F.).

3. In the mosquito the pigment of the spheres seems more scattered and much more alive than in finger-blood ; while almost all the spheres are in this swarming condition when the specimen is first made, whereas in controls many motionless spheres are seen.

4. In the large number of controls [of finger-blood] examined carefully by me I have only seen 3 flagellate organisms (1 yesterday) (temp. 90 to 100° F.) ; whereas out of 11 good mosquito specimens I must have seen sixty or seventy at a modest figure,

[1] Because blood in mosquitoes is more concentrated owing to absorption of the fluid by the insect's digestion.

and these, all but one, in three mosquitoes 18, 21, 24; and might have seen many more had I rushed through the specimens quickly enough. On two occasions I saw five flagellates in a field.

5. The phenomenon of ex-flagellation appears to be going on when the specimen is made and certainly ceases a few minutes afterwards. I have not yet watched a single spherule becoming flagellate, however ripe it has appeared for that development.

I must now cease, Dr. Manson, as I have numbers of letters to write; but cease with regret because I could write much more. I propose for the present to go over and over these points as opportunity affords, but alas! the wonder case which fortune has sent me is *drying up*, and I may be ordered off to Patiala in a day or two. I may tell you that after seeing mosquito 18, and much more after mosquitoes 21 and 24, I *felt* that practically the first step was assured; but I don't care to be incautious. You can imagine my excitement during the week; yet as I have been able to make step after step I have had a presentiment of what was coming. Of course the principal excitement was over the flagellate organisms. I *knew* that I would get them in 18 and still more in 21. The *first* mosquito of a batch examined will I think nearly always yield flagellates, the rest none—why? But the mosquito ought to be killed within half an hour. I leave you to judge as to value of evidence; but must state that every point that you predicted seems to come true; certainly there is nothing contrary to the theory and certainly neither iced nor deep control specimens show anything like the same degree of crescent-sphere-flagella metamorphosis as is seen in the mosquito. I look upon these observations to be as much yours as if you had made them[1]; and if you think they will in any way advance the first steps of the mosquito-malaria theory please make use of them. They are facts but require sifting. Presently I shall give infected mosquitoes, and the water they have died in, to natives on payment and see results; then for seeing where the flagella go to. You will be very pleased with the following. The Maharajah of Patiala in the Punjab (whose medical officer Surgeon Major Owen, C.I.E., C.M.G. I wrote to) has written officially to the Government asking for my services from 1 June to 1 November to investigate the fevers which abound

[1] They could all have been made in England.

in his country! What an opportunity. But Government may possibly object: in which case I will ask you to help me in the B.M.J.[1] I received a very nasty letter from head-quarters which knocked me over on 21st.[2] I have found another typical spring tertian in my hospital. I laughed at Hehir for his 30% infected corpuscles; but see that Marchiafava notes 50%.

Kind regards to Mrs. Manson.

Yours truly,

R. Ross.

Are you trying gnats at home? Is any one else working at mosquito? [compare page 131].

SECUNDERABAD, 28 *May* 1895.

DEAR DR. MANSON,

I think that my last letter missed the post owing to the date of sailing having been advanced one day without sufficient notice; I am not sorry, because my former observations were somewhat erratic, and I have now been able to put in five days of more methodical and satisfying work. I have been going over and over the same ground and have been able, I think, to reach some definite conclusions respecting the first step of the metamorphosis inside the mosquito.

After writing I determined to follow your advice exactly, get a batch of full mosquitoes and examine one after the other at short intervals. On the 24th I obtained a batch of six mosquitoes all of which had begun biting nearly at the same moment, that is, directly the patient was put into the net. They were now dissected and examined with the utmost rapidity possible at intervals of 8, 22, 40, 60, 120 and 180 minutes after they had begun to bite. The results were. [Details of mosquitoes 29 to 34.]

The above is taken from notes made between each mosquito. Before beginning the process I made a sealed control specimen from the finger which, together with mosquitoes 29–32 I re-examined whenever I had time in order to watch and compare progress, *pari passu*.

[1] Manson had informed me that he was adviser for tropical diseases, or something of the kind, to *The British Medical Journal*.

[2] I had applied for civil employment, but not through the "prescribed channel."

25 May. The process repeated. [Details of mosquitoes 35 to 40.]

The above, with all my former experiments, had now given me a very good idea of what occurs in mosquito's stomach. Sunday 26th I set aside for experiments with haemocytometer —of which, later. On 27th and 28th (today) the whole course of the 24th and 25th was repeated to make absolutely sure.

The result is that I can now make a definite statement of the life history of the parasite from its being drawn in to some hours later; and this with the greater certainty because the experiments invariably confirm each other with but trifling differences.

. . . The progress of things, then, is this.

1. Almost all the crescents are converted into spheres very shortly after they enter the mosquito's stomach. Sometimes a fair number may-be found at 10, 15, 20 minutes or even more; but these are almost always *seen in the same part of the specimen* and therefore belong probably to the *last intake of blood*. It is very rare to find more than *one* or *two* crescents in a whole specimen made at a longer interval. Ovals are not often seen. Untransformed crescents are I believe injured or aborted ones.

2. The spheres are always found. They have a perfect contour and contain a clear fluid which distinguishes them markedly from the masses of corpuscles in the midst of which they are generally found. At first their pigment is clustered together; then the outermost pigment particles begin to oscillate; lastly all the pigment particles commence swarming, while the whole cell may take on a slight jerking rotatory movement.

3. Flagellate organisms may be found from 7 to 38 minutes or more. When seen, they lie almost always in great numbers in *contiguous fields*, suggesting that they belong to the *same intake* of blood and have *ripened* at the same time. They are always, so far as I have experienced, found within a minute or two of the lens being brought to a focus (my first survey being very rapid). The whole manifestation ceases in a very few minutes. Owing to this last fact, and possibly owing to the *process of making the specimen allowing the flagella to free themselves*, they are sometimes not to be found (?)

4. The spent pigment masses [1] may sometimes be seen very early, even at 8 minutes, but then are *very few* in number. This number steadily increases, almost without fail, in successive mosquitoes until, as in mosquito 39, they may reach 65% of all the parasites (not enclosed in phagocytes). Their appearance is quite characteristic, simply because they are not enclosed in the clear cells of the spheres; compare [drawing] and [drawing] or [drawing] or [drawing]. They are easily seen to be mere dead clusters of pigment (sometimes with very feeble oscillations). They may be masses, rings or *drawn out* clusters. Their absence in the earliest mosquitoes and their large numbers in the latest show conclusively that they are due to a progressive metamorphosis inside the mosquito and not to *crushing* in making the specimen, but many of them may be pigment *rejected* by phagocytes which have swallowed spheres; I think Mannaberg allows this.

5. Phagocytes containing spheres and pigment begin to be seen later than the spent pigment, at about 20 minutes or so; and increase in number up to about 2 hours.

6. About 30 or 40% of the parasites *fail* in *throwing out flagella* and remain as spheres,[2] the pigmentary movements gradually becoming slower and slower, the pigment collecting in a heap to one side and then (2 hours) the bioplasm beginning to swell up irregularly (Laveran's old corps, No. 3).

The above developments may be shortly stated in two sentences—

A. Practically *all* the crescents become spheres a few minutes after being taken into the mosquito's stomach.

B. From 30 to 40% of the spheres *die* after 1 or 2 hours; the rest having either given out flagella, been eaten by phagocytes, or having simply broken up (?).

29 May. These observations, then, prove conclusively that the crescent-sphere-flagella metamorphosis *does* go on inside the mosquito; which I think you said was to be considered as the first step in the theory. But it may now be said with reason that this does not prove that the stomach of the mosquito is the *natural locus* of the metamorphosis, because a

[1] That is, the crescent-sphere (Plate II, fig. 19) *after* the flagella have escaped from it, leaving only a number of pigment granules within a collapsed little bag.

[2] These are now known to be the *females*. I thought that they were parasites which had died without throwing out "flagella" (sperms).

similar metamorphosis may be seen to occur whenever the blood is drawn from the patient and allowed to remain still for a sufficient period. Against this objection, I will state that the *metamorphosis proceeds to a much greater degree in the mosquito than in control specimens of finger blood.*

1st. The number of crescents, which in a finger-blood specimen, become spheres varies much in different specimens made at the same time and also in different parts of the same specimen. I think you will agree with me that from 10 to 70% of the parasites remain crescents. Thus this morning I made two good controls; in the first of which the crescents were only about 10% after 35 minutes; while in the second made shortly afterwards the crescents numbered from 60 to 70% after half an hour. In short it seems that the crescent-sphere metamorphosis is greatly influenced by certain local conditions of the specimen[1]; but it may be stated confidently that a very fair number of undeveloped crescents are always to be found in finger-blood, even after the lapse of many hours. This is not the case in the mosquito; here, practically *no crescents* are to be found after half an hour or so. In fact I am prepared to state that I can distinguish at sight between a specimen which has been made directly from the finger and one which the blood has passed for 6 or 10 minutes through the mosquito's stomach.

2nd. I have remarked already on the greater violence of pigmentary movements in the mosquito.

3rd. Since my last letter I have found flagellates in control specimens much more readily because I now know exactly the period when to look for them (10 to 20 minutes). Still I have never seen anything like the numbers witnessed in mosquitoes 18, 21, 24, 44, 46.

4th. A curious proof from the effect of *quinine.* Patient had 15 grs. on 27th and 10 grs. yesterday morning. In my preliminary control of yesterday I was astonished to find hardly any of the crescents transformed, and, since I had seen the same before, after quinine, I thought that all the crescents had been killed. On examining my first mosquito at 10 minutes, I again found all the parasites *crescents.* This was quite contrary to all former observations; and I was now getting certain

[1] See page 195 for the reasons.

that the crescents were indeed dead, until, on examining my next mosquito at 20 minutes, I observed that the proportion of spheres was much greater, though crescents were still much commoner than they ought to have been. Successive mosquitoes gave fewer and fewer crescents, until after 69 minutes there were only three in the whole specimen, while the sphere-pigment metamorphosis had gone on as before. Returning now to my original control, I found all the crescents still there and no pigment masses. From which I conclude :—Quinine *checks completely* the crescent sphere evolution in finger-blood : but only *delays* it in the mosquito. In other words, the mosquito's stomach has the power, apparently, of resuscitating parasites paralysed by quinine. Of course the effect of quinine on flagellate organisms has long been given ; but it is generally stated that it has no effect on crescents.[1]

On Sunday 26th I made some laborious attempts to estimate number of parasites in mosquito blood with hæmacytometer. These all failed owing to difficulty of obtaining *enough* blood and dryness of air. I estimated parasites in finger blood at 1 to 410·3 corpuscles ; that is, 8,000 parasites and 3,282,400 corpuscles in cubic millimetre of blood (my first attempt was computed wrongly). Number of parasites still appears to me to be generally much greater in mosquito : but I cannot get proof.[2]

I have only seen one free flagellum, but some curious things. Do you think it possible that all the flagella are coils of a single long animal inside the sphere (?). Hardly possible. To-morrow I begin second step—watching the flagella. Please send me advice. I watched a sphere this morning break up and become spent pigment apparently without exit of flagellum. Parasites continue decreasing slightly in numbers. Have dissected 54 mosquitoes up to date. My Parke's Gold Medal arrived. No further news from Patiala. Three more cases of Tertian parasite in my hospital. On 25th I made a *certain* experiment on two men ; of which you may hear later. Every one of your predictions has come true.

With kind regards,

Yours truly,

R. Ross.

[1] I became less positive on these points later.

[2] See footnote, letter of 22 May 1895, page 145.

CHAPTER X

FALSE SCENTS. SECUNDERABAD, 1895

THUS within one month I had been able to show that the crescent-sphere-flagella phenomenon occurs in the mosquito's stomach, not only just as it does in blood from the finger but much more frequently and simultaneously—suggesting that the mosquito's stomach is the natural *locus* for this surprising metamorphosis.

During my stay in India Manson wrote me fifty-five letters, from 17 May 1895 to 18 January 1899, exactly half the number I wrote to him. I should like to print all of them, but do not feel justified in giving more than a few extracts directly concerned with my work. Here are some from his first two replies.

21 QUEEN ANNE STREET,
CAVENDISH SQUARE,
17*th May* 1895.

MY DEAR ROSS,

I was delighted to get your letters from Malta and from Secunderabad and to gather from them that you pursue the plasmodium with enthusiasm. You do not lack for material and energy, and though you may get many a check I firmly believe you will work out some important result; you cannot fail to do so. Let this conviction fill your mind when inclined to despond, and look on a stone wall as a stimulant rather than as an insuperable barrier to progress. I have run my head against them often enough, but somehow if I kept hammering at it and thinking about it sooner or later I got over or under or through when I had set my mind to it; and so will you. . . .

Already you have proved that Crombie is wrong and that a diagnosis of malaria can be made with the microscope in less than five minutes. You must set them right about this in the first place. . . .

Soon I expect to hear that you have followed the plasmodium

into the mosquito or some other suctorial insect. If you succeed in this you will go up like a shot and get any facilities you may ask for in the way of leisure for investigation and laboratory appliances and assistance. I only wish I were you.

I am full up with preparing lectures for my course on tropical diseases at St. George's. I hate the labour of writing, but I am in for it and have got to get it done somehow. My first lecture is next Tuesday. I have promised twenty—two a week —but so far have only some 8 or 9 ready. I wish they were all written.

In consequence of this work and a good deal to do in the way of practice I have not had much chance for further work at malaria. Indeed patients have been few for the last two months. I got a guinea worm case some time ago and have on hand a rare lot of infected cyclops.[1] It is really a very pretty experiment. I had some photomicrographs made of the cyclops by Pringle. They show the embryos in the interior of the cyclops very well indeed. I shall try to get a spare one to send you one of these days. . . .

Bakers' are going to lend me six of your microscopes for my lectures. They will be well advertised therefore and I shall show my gratitude to you and to Baker by giving them a puff. He has already sold a considerable number and is now making a dozen [page 131].

Hoping to hear again from you soon, and assuring you of my sympathies in your work, and willingness to help you in any way I can,

I remain,

Yours very truly,

PATRICK MANSON.

21 QUEEN ANNE STREET,
CAVENDISH SQUARE,
21 *June* 1895.

MY DEAR ROSS,

I am exceedingly obliged for your letters. I look forward to receiving them with the greatest interest, and when a mail passed without getting one the other day, I was terribly disappointed for I thought you had fallen sick, or that you had got a check, or that you had given up the quest. Above

[1] Fedschenko's cycle, see page 127.

everything don't give it up. Look on it as a Holy Grail and yourself as Sir Galahad and never give up the search, for be assured you are on the right track. The malaria germ does not go into the mosquito for nothing, for fun or for the confusion of the pathologist. It has no notion of a practical joke. It is there for a purpose and that purpose depend upon it is its own interests—germs are selfish brutes.

Your finds are most interesting and encouraging. The mere fact that the parasite is not digested is enough to prove to my mind that it is in its proper habitat, and the fact you have discovered that it advances in development more rapidly and more frequently in the mosquito's stomach is confirmatory of this inference. But now the struggle to advance the next step begins. What next to look for and where to look.

I think you would do well to prepare a series of stained preparations from mosquito blood illustrative of what you have already done. I think that such a series of preparations would show the progress of development better than anything else as they could be made so rapidly at a few minutes interval and could then be stained and examined at leisure. If you have time do this and if you are generously inclined send me a series either in bottle or stained or both. . . .

About the next step. Among other things examine the dung the little yellow specks you see on the glass the insect is kept in for evidence of malarial products. It may contain nothing but it is easily examined. Then I should examine the ova of the mosquito from time to time before and after they are deposited in the water, for I have an idea that the beast gets into them and so into the larva and becomes more or less a permanent parasite not only in the individual mosquito that sucked him in but in the progeny of this mosquito. This will be a more difficult matter. Then I should keep mosquitoes to maturity and tease them up in salt solution and examine their tissues one by one searching inside all cells for something like the plasmodium. I question if he will be pigmented but I am fairly sure that he is intracellular. This too will be a long and difficult task. But it is not without hope. Look about the kidney tube, or the analogous structure, among the long cæcal tubes, about the little corpuscles representing the blood corpuscles in the walls of the stomach in the muscles of

the thorax. Compare malaria fed mosquitoes with non malaria fed mosquitoes and compare their progeny.

If you would experiment create an artificial marsh by allowing the water in which the mosquito has died and laid her eggs and in which her progeny have been swimming about to dry up, then pulverise this and get someone to inhale the dust you stir up from it. Or drink it suspended in water. The experiments would be interesting but perhaps not altogether safe. Dulce et decorum est pro patria mori. But dont do it yourself.

My guinea worms are still alive in the cyclops and slowly undergoing changes. It is now over six weeks since they entered the crustacean and they are still quite lively only casting their skins and dropping their tails according to last report three days ago. I hope you will have a chance of a fling at this subject as well as at malaria and filaria.

I must close my letter now as I have a lot to do this afternoon and dont want to miss the post or to let you suppose that I dont take a great interest in your work, and to give you an excuse of not writing to me.

With all good wishes for continued success,

Yours very sincerely,

PATRICK MANSON.

This last letter reached me before 10 July, by which time, as will be seen, I had already begun to try to infect human beings by water from an "artificial marsh" and to search the tissues of mosquitoes for parasites similar to the *Plasmodium*—it was too early as yet to search their eggs and evacuations. Of course all these suggestions were obvious to anyone who was thinking seriously of the subject, and Manson seems to have forgotten that we had already talked them over in England.

Thus far Manson's induction had carried me triumphantly along, but it already began to fail me as a clue. He had supposed that the so-called flagella (Plate II, fig. 21) are flagellated spores, that is, actively motile bodies which could work their way through the stomach-wall of the insect in order to live in the tissues, where, he thought, they grow in some unknown position into an unknown form. Still later, he surmised, they escape from the insect into the water when she dies on that fluid and thence infect any person who happens to drink it. But if all this had been true I should now have been able

to observe large numbers of the alleged flagellated spores wriggling about everywhere in the contents of the stomach. I was much disappointed at not finding them, though I did not like to disappoint Dr. Manson equally by telling him so at this early stage of the investigations (but see letter of 10 July). I still hoped that something curious happened to the flagella which enabled them to escape my oil-immersion lens. That indeed was the case; they are not flagellated spores at all but sperms (Plate II, fig. 22)—but this was not found out for two years more.

In the meantime, however, I had thought of a short cut. If Manson's hypotheses were sound (and the mosquitoes were of the proper species) water in which a mosquito which had bitten a case of malaria happened to die should infect healthy persons after a few days; and I had already tried the experiment—as will now appear. It is an example of the tricks which fortune plays upon investigators that my first experiment appeared successful. But it was a mere accident—many subsequent cases failed entirely to react.

The mosquitoes with which I was working were either my *grey* or *brindled* mosquitoes (see page 219), that is, *Culex* or *Stegomyia*. As I have said, I could obtain no literature on mosquitoes at all at that time even when I was in England except a non-zoological article in an encyclopedia. The large majority of the insects used in the above experiments were *Stegomyia*.

A paper by me describing all this work [17] was subsequently read before the South Indian Branch of the British Medical Association in December 1895 and was published also in *The Indian Lancet* 1896. Neither of these publications is easily accessible in this country or abroad.

SECUNDERABAD, 5 *June* 1895.

DEAR DR. MANSON,

I am in a state of great excitement and have no time to write you a full letter this week as the post goes out today. One of my "certain experiments" which I mentioned has come off.

On 17 May I took four full mosquitoes from the crescent case, Abdul Kadir, and put two in one bottle and two in another with a little water. They were kept in a cool place until 25 June [May], when they were found dead on the surface of the water. In one of the bottles very small mosquito grubs

were found, showing that the mosquitoes' eggs had been hatched. The contents of this bottle, minus the dead bodies of the mosquitoes, was given to a native, Lutchman, on 25 May at 8 p.m. on payment, after full explanation of the nature of the experiment. The contents of the other bottle was given to another native in a similar way. I think myself justified in making this experiment because of the vast importance a positive result would have and because I have a specific in quinine always at hand.

Lutchman is 20 years old, a healthy looking young fellow who *says* that he has never had fever. Yesterday he was all right. This morning at 8 a.m. (exactly 11 days from drinking the water) he came to the hospital-assistant complaining of headache. Temp. 99·8. Looking ill. Temperature has been taken every half hour and rose to 101·4 at noon, since when (1.30) it has been declining. This looks very promising. Incubation agreed closely with former experiments. The type ought to be either quotidian or summer tertian according to Marchiafava, etc. (crescents), which do not necessarily begin with rigor. Headache considerable. Three specimens carefully examined give no results; but such are not yet to be expected. If temperature rises again this afternoon it may be summer tertian. Chances are greatly against accidental fever, because out of the whole regiment I am getting only 3 or 4 cases weekly; these are always either spring tertians or *no parasite* (not carefully examined). Anyway, if the attack is quite ephemeral, the result will be interesting. A third man drank mosquito water on 31 May. . . .

Yours truly,

R. Ross.

The letter concludes with some work confirmatory of previous results and a postscript on Lutchman's temperature on 5 June, taken half hourly up to 2.30 p.m.

SECUNDERABAD, 10 *June* 1895.

DEAR DR. MANSON,

Many thanks for your letter of 17 May—but first about the man Lutchman. He had three days fever, but I failed entirely in finding any parasites in his blood. His temperatures were. . . .

[Here follows a table of hourly temperatures from 8 a.m. to

midnight, taken on 5, 6, 7 June. The highest temperature reached was 103·0° F. at 5 p.m. on the 5th, and 100° F. on the 6th; after which it fell to normal except for a very slight rise on the 7th.]

This gives three rises—at 8 a.m. on 5th, at 3 p.m. on 6th and at 11 a.m. on 7th. The first attack lasted 20 hours. Clearly it was intermittent fever possibly of a mild *postponing summer tertian* type. There was headache, but neither rigor nor splenic tenderness. Bowels constipated first day. No medicines given. Repeated examinations of the best possible fields of flattened smooth round corpuscles failed to show any parasites at all. . . .

12 June. No parasites in Lutchman's blood and no more fever. Neither of the other men has had fever. I am at present, as ill-luck has it, getting no cases of parasites and so have been dissecting a mosquito or two, pricking fingers at random and playing golf. The rains have broken, and it is only 88° in the verandah at noon. I found a beautiful double tertian some miles from here; but the man was too ill to be moved. The doctor asked me to look at the blood as he thought it was a *funny case*. . . .

In several mosquitoes I find bodies like this drawing [see fig. on page 169] which seem to me to be parasites, perhaps coccidia. They are about 20 μ in length and were very numerous. . . .

Do you think that you would like to come out to Patiala instead of to Demerara [page 162]? I should like to sound Owen about it, if I go there. . . .

In specimens made after your method [Laveran's method] we get five different kinds of fields which I find it very useful to distinguish thus :—

Field A. Very thin. The hæmoglobin is crushed out of the corpuscles and forms a thin homogenous yellow fluid containing only shadows of corpuscles.

Field B. A little deeper. All the corpuscles are smooth, not crushed nor vacuolated, lying separate or tesselated; the serum is clear. This is the best field for intracorpuscular forms.

Field C. The corpuscles lie fairly separate but are crushed and vacuolated owing to no pressure being exerted on them by the cover-glass.

Field D. The corpuscles are in rouleaux. Good for crescents and crescent-sphere-flagella metamorphosis.

Field E. The marginal clot of drying-blood. Good for all sorts of pigment masses.

Have you ever seen a phagocyte take in a whole swarming sphere and burst it, as we burst grapes between the tongue and palate ? I have, several times. It is done in an instant. Another point—the quinine and fever forms described by Mannaberg occur, I think, only after the specimen has been made for some 20 minutes.

With kind regards, believe me,

Yours truly,

R. Ross.

I shall write you again when I have *something to say.*

The parts of this letter not printed give a tabular summary of my results with the man Abdul Kadir, which were described in previous letters. The parasites in his blood now began to diminish rapidly, and I was forced to search elsewhere for cases.

SECUNDERABAD, 26 *June* 1895.

DEAR DR. MANSON,

. . . I brought back Abdul Kadir to my hospital since he still has a few crescents but I was allowed to have him for only two days. I went through the " course " twice however. It appears useless to repeat this process on the man (I have done it 8 times now) because the results are invariable in every experiment. The man has been having much quinine lately and consequently the crescents scarcely transform at all in finger-blood specimens. They remain unchanged for an hour or more (temperature of air 75° F.) and then perhaps one out of four or five becomes a sphere. I saw one flagellated body with 8 distinct flagella. What now is so remarkable is that in the mosquitoes they are *all* transformed in 20 minutes at most. In six mosquitoes containing about 15 to 20 parasites each, I found only *one* crescent—all the rest were spheres or spent pigment. In these cases I examined the mosquitoes as well as the controls most thoroughly and deliberately and I am absolute *certain* now of the great effect produced by the insects stomach on the crescent-sphere transformation, at least so far as I can judge by one case alone (Abdul Kadir). It is a remarkable and significant fact,

There is much to be studied in the ex-flagellation process (shall we call it ecdysis ?). In the prisoner I watched a sphere throw out flagella which almost immediately curled up round the cell and stopped moving (certainly within 120 seconds). Mannaberg describes this somewhere. The extreme brevity of the stage in this case shows why one so often misses flagellated organisms. I saw none in my last mosquitoes though spent pigment masses abounded and showed that the process was occurring. When they *are* found I think they are generally found in *clear* serum. This suggests that in viscid blood the flagella are able to leave the cell so rapidly and easily that scarcely a movement is seen—what do you think ? I remember that in mosquitoes 18, 21 and 24 nearly all the flagellate bodies were in pretty clear serum. But why do the flagella sometimes curl up so quickly ? Also why do the cells sometimes break up without any flagella at all ? Have you seen this ? Why have I seen so few free flagella in the mosquitoes ? [1] . . .

. . . in May and June I have found altogether 6 spring tertians, 2 quartans, 3 summer tertians, 5 crescent cases and one with an undetermined fever form—17 cases. What has made me rather conceited is that most of my 20 negative cases mentioned in postcard turned out to have a curious *continued* fever lasting 4 or 5 days. They all began in a batch after the first heavy rains. On the whole I don't think I can have actually missed the parasite in more than four or five true malaria cases—and this though I made no persistent search. In fact I am inclining to the view that when the parasites are present they can usually be found in 2 or 3 minutes at most. . . .

Have got six mosquitoes from Abdul Kadir maturing in order to give to a man. About 15 or 20 parasites in each. I shall give them all together. No crescent in Lutchman, nor any more fever.

Yours truly,

R. Ross.

SECUNDERABAD, 10 *July* 1896.

DEAR DR. MANSON,

During the last fortnight I have not been able to find a single fresh case of malaria though I have been hunting all round the station for them. . . . I have looked in all sorts

[1] Because many of them were entering the female spheres at once.

of cases for crescents without result. In spite of offering rewards to hospital assistants of the civil and other hospitals I can't get a case. I thought I should find cases in the civil hospital the other day to a certainty, but was disappointed. I don't know what's up with the place. Yet there are tanks and marshes everywhere and only a scanty rainfall. The huts of my regiment are practically just over a great tank with muddy, drying margin—yet out of 800 odd men, no malaria. I will tell you why—because the mosquitoes don't thrive this windy weather. I will bet on your suggestion that malaria is in the first place a disease of mosquitoes—hence the idea of it being carried by winds. It is the *mosquito* and not the *germs* which are carried by the wind. They probably deposit the germs in the drinking water. This is the reason why I am beginning to work with drinking water instead of with an artificial marsh as you suggest. If the germs were so numerous in the air as to infect one person they ought to infect *many*; this will not explain isolated cases satisfactorily. It appears much more likely that only those persons are infected who have drunk water by chance out of a chatty or water pot in which an infected mosquito has laid her eggs—the infected mosquito may of course have been blown from a marsh, where the disease is rife among the mosquitoes, or may have recently bitten a malaria case. During windy weather mosquitoes don't thrive. . . .

Why did not I find more *free* flagella ? Either they dash away into the tissues at once, or, I am beginning to think, cease to move almost as soon as they free themselves from the cysts (footnote, page 160). They have to scurry out of the cyst for fear of the phagocytes; but after that may develop without movement in the contents of the stomach—again what do you think ? Oh for another case ! I make this conjecture because there ought to be numbers of free active flagella in mosquito's blood, whereas I saw only two or three; while on the other hand they may be seen to cease moving in a few minutes. In mosquitoes 18, 21, and 24 there ought to have been swarms of free flagella apparent *if they had continued wriggling*. I think that this point requires study before attacking the question of the tissues; I shall work with much thinner films than, for fear of bursting the spheres, I did before. Don't

you think that the flagella ought first to be found free in the stomach? Staining etc. must be tried. As soon as this is done I shall go on to follow your advice exactly about the tissues. . . .

Tomorrow morning I am going to see some horses said to have intermittent fever. Am thinking of sending an account of the mosquitoes to the South Indian Branch of the B.M. Association in order to work them up a little. Nobody is doing anything in the microscope line. The Patiala investigation has collapsed entirely owing to opposition of Lieut. Governor of Punjab. . . .

With kind regards,

I remain,

Yours truly,

R. Ross.

The Patiala business was as follows: Surgeon-Major Charles William Owen, I.M.S., C.I.E., was Chief Medical Officer to the Maharajah of Patiala, the head of the Sikh nation in Northern India, and was interested in malaria and had corresponded with me before I left for India. On or before the time I reached Secunderabad I had suggested to him that the Maharajah might confer a benefit on medical science if he were to apply for my services for the purposes of a special malaria survey in his territory. Of course such a thing at that time had hardly ever been done; but Surgeon-Major Owen, with great enterprise, persuaded the Maharajah to approve the scheme, and I was expecting to be sent north any day, when there came the further news that the Lieut.-Governor of the Punjab had vetoed the proposal on the ground of expense. That ended the matter. In fact I was so favourably placed at Secunderabad for malaria work that I would not have gained much by going north, except money, regimental pay being very small, but I did not know this at first.

The reference to Demerara in my letter of 10 June may also be explained. When I was in England, Dr. Manson told me that he was proposing to go there for the purpose of working at his mosquito theory himself if only he could have his expenses paid by the Royal Society or some other body that grants funds for such purposes; and I now suggested that perhaps Patiala would take him instead of myself. In his letter of 10 July, however, he told me that the Royal Society declined to give him the grant asked for. All the same I always wondered

why he did not make the required studies himself at the Branch Seamen's Hospital at the Royal Albert Dock, of which he was a physician. There were plenty of malaria cases there and malaria-mosquitoes (*Anopheles*) abound in the neighbourhood; and indeed I suggested this again in the postscript to my letter of 22 May. As a matter of fact he could easily have done in London all the work that I had been doing in Secunderabad—incubators, mosquitoes, and suitable cases all being available. He had I think hoped to prove the theory in a few months' work at Demarara, but, as will be seen, he would have found the job a much tougher one than he had expected. The British Medical Association had actually granted him £150 on 16 January 1895 for the proposed visit (see *Br. Med. Journ.* 29 April 1922, p. 698).

That Manson himself was thinking of working at his mosquito-theory in London is apparent from the concluding extract from the following letter (his third one):

28 *June* 1895.

My Dear Ross,

. . . My idea about the beast in the mosquito is as I think I told you that it is transmitted in a sort of hereditary way from mother mosquito to sons and daughters as a cell parasite, that it gets to man in mosquito dust—the débris of dead mosquitoes in which the germ has become encysted, that these germs on entering the human mouth, air passages, or swallowed in the saliva or what not, or as in your experiment in water, penetrate the human mucous membrane as they do the mosquitoes stomach, and so get into his circulation. An experiment I should like to see tried would be to evaporate some water in which malariated mosquitoes had died—say in some small glass bottles, to evaporate it thoroughly, so that the sediment will not decompose, but to evaporate it in the sun so as not to scorch it, and also so as to resemble what occurs in nature; to have this transmitted to England; to add there a little water to the dried sediment and then to drink it. I am willing to try. A positive result to such an experiment would be irrefutable. Another experiment worth trying would be to pulverise this same sediment or the bodies of desiccated malariated mosquitoes, and then inhale the dust.

A suggestion I would make about these experiments is that the mosquito water or mosquito dust, should be taken or

inhaled first thing in the morning and on an empty stomach so that there would be no danger of its having its included germs destroyed by the stomach juices.

Pay some attention to the mosquito grubs in malariated mosquito water; it may be that they take in the germ in the water they swallow.

Another hint I would give you. Send specimens of the mosquito for identification of species. Possibly different species of mosquito modify the malaria germ; that the differing degrees of virulence depend on the difference of species of mosquito that has served as alternative host to the parasite. . . .

Simplicity is the best guarantee of success. As little machinery as you can get along with. Let the other fellows, the men who are going to come after you, worry with these things and elaborate in detail the big principles which you indicate and establish. Go for principles—not details too much.

I have not had a malaria case for a month. I hear of some mosquitoes at the docks and if I got my case I should have a try for the malariation of the insect and the consumption of his infusion myself. . . .

I remain,

Yours very truly,

PATRICK MANSON.

SECUNDERABAD, 17 *July* 1895.

DEAR DR. MANSON,

Your very pleasant letter of 28 June reached me only yesterday. It is very encouraging to me to get your letters; but you *must not* feel called upon to waste your time in replying to mine unless you have absolutely nothing better to do . . . remember that I am an exile with no one to talk to about malaria germs! but you must not waste your time over replying unless you think it necessary. . . .

I am nearly wild, and have been able to do nothing for nearly six weeks now. I have been all round the hospitals again and have touts out everywhere and, with great difficulty have obtained three bazaar people—no crescents. The bazaar people won't come to me even though I offer what is enormous payment to them. I offer 2 and 3 rupees for a single finger prick and much more if I find crescents—they think it is witchcraft. . . . Also, do you know, I believe that I have come upon a

new variety altogether; but have not observed it sufficiently yet to let you know about it; the corpuscles seemed to be infected with 1, 2, or 3 very minute forms. . . . Perhaps after all they are some kind of vacuole—but they move and turn round!—we will see. [What I called "eye-forms" later. Of no consequence (page 189).]

I also examined five horses with fever—chart not intermittent—no parasites seen.

The fourth *experimental case* has had no result (now 15 days). But the malarialised mosquitoes contained only a few parasites each and were kept too long dead I think. Thus only one out of four cases contracted fever; and though that fever *looked* like a malarial one (summer tertian—not clear), I could find no parasite. Yet the incubation period was quite correct and when we come to think of all the chances of failure in such experiments we must by *no means* expect success in more than a small percentage of cases, at least at first. . . . Don't for heaven's sake mention at the B.M. Association that Lutchman is a dhooley-bearer—did I tell you so?—he is a Government servant. To give a Government servant fever would be a crime! (Also, I will add here, to give a lecturer, on tropical diseases at two London Hospitals, guinea-worm would be a worse one.)

You must not think of trying the cyclops. What I suggest is this. Bribe the man at the Bar at the House of Commons to put the cyclopses in the drinks of the M.P.'s. It will draw attention to science, put a stop to much useless talking by keeping the M.P.'s off their legs, and not injure anyone of consequence.

With kind regards,

Yours truly,

R. Ross.

SECUNDERABAD, 18 *July* 1895.

DEAR DR. MANSON,

At last!

Just as I said in my letter yesterday, Abdul Wahab, the man with ring-forms and pigmented leucocytes only, showed crescents this morning—1 in 4 fields. At work again. Eight hours today.

I said I was going to watch free flagella [Plate II, fig. 21].

With my usual luck I found a beauty in my very first specimen and watched it for three solid hours exactly, without taking my eye off him. So much for my absurd theory of the flagella becoming quiescent. What he did you will think interesting.

He riggled [*sic*] around for 20 minutes like a trypanosoma, so that I could hardly follow him. Then he brought up against a phagocyte and remained so long that I thought the phagocyte had got hold of him. Not a bit; he was not killed or sucked in; but kept poking him in the ribs in different parts of the body. I was astonished; and so, apparently, was the phagocyte. He kept at this for about ¼ of an hour, and then went away across two fields and went straight at another phagocyte! He pushed into this in several places with one end for a long time; and the phagocyte seemed to rear up and try to get round him, but could not. At last the phagocyte seemed to give up and flattened itself against an air-bubble, the flagellum still poking away at him. After 50 minutes the beast seemed to be getting tired, when a very curious thing happened; a third phagocyte came at him with mouth open right and straight across the field, but had no sooner got near him when the flagellum left his fallen foe and attacked the new one, holding on and shaking like a snake on a dog. In one minute the third phagocyte turned sharp round and ran off howling!!! —I assure you. I won't swear I heard him howling, but I *saw* him howling. He went right across a whole field, the flagellum holding on to his tail like this [drawing]. This continued for five minutes, the poor phagocyte literally *legging it*, after which the flagellum left him and went away. By this time the beast had become more visible and had a large swelling in the middle [drawing]. I watched him steadily, and his movements gradually became slower. He was certainly able to attach himself to the cover-glass (as shown by touching it with a pen) by *either* end and even perhaps by the swelling. At last this swelling moved to one end [drawing] nearly of the beast and became very large and distinct, until after three hours the creature evidently died—at any rate, curled up and ceased to move. Isn't this interesting? I think it shows, first that the phagocytes have no more power over free flagella than over trypanosomes; secondly the beast was evidently attacking the phagocytes, probably *mistaking them for some other*

kind of cell [1]—so I judge by its leaving the phagocytes after a time.

After this I put the mosquitoes on. Results of Abdul Kadir completely confirmed—and more. The first mosquito was killed at twenty minutes. My usual luck again—*all the parasites were flagellate and there were scores of free flagella.* I say *all* the parasites—all except about 5% which were spheres. In the second mosquito at 40 minutes, I saw four flagellates; the rest were spheres and spent pigment. In neither mosquito was there a single crescent, though in the first control specimen 50% of the parasites remained crescents after three hours. I killed no more mosquitoes, being tired out, but kept 8 for malarialising experiments. After this it is not much use bothering about going over the "first step" again—but I shall make you some sets of specimens and try to stain the flagella; Mannaberg does it. The spectacle of the first mosquito was really wonderful. I shall dream of it. Good night. So was the fight between the flagellum and the three phagocytes, I shall write a novel on it in the style of "The Three Musketeers." . . . [24 July]. Numbers of perfect rosette forms found yesterday [in a case of quartan]. The man saluted, said he was quite well and fit for duty. "Wait a minute, my friend" I said "you will have a bad attack of fever presently." "Oh, no," he said, "I am quite well." I told him to wait; in an hour his teeth were going like castanets. They now take me for a prophet. I got the mosquitoes on to the two other crescent cases.

You may now feel assured that the "first step" is completely proved. I have tried four cases with identical results.

With kind regards,

Yours truly,

R. Ross.

SECUNDERABAD, 30 *July* 1895.

DEAR DR. MANSON,

. . . A very useful way for finding crescents and pigment, suggested by your marginal clot and the mosquito's stomach in which pigment is found almost instantly. Put

[1] Absolutely correct. I think it mistook the leucocytes for female *Plasmodia*. Detailed notes and drawings of all these observations are entered in my laboratory diary.

a large drop of blood on a slide without spreading it out and stir it with a needle and breathe on it for two minutes until it is half dry; then put on the cover-glass. The corpuscles are found to be in a viscid yellow mass in which the black pigment of the crescents is seen in a moment. We may thus examine a large quantity of blood in a very small area. This method enabled me to find crescents at once where I had failed after a long study of two ordinary specimens. Useful for diagnosis.[1]

On 26th. I put three mosquitoes on my quartan. Numbers of intraglobular forms, beautiful rosettes, but no flagellate form seen. (Morning before fever.)

Same day one *flea* on Abdul Wahab. Only four parasites found in the whole insect, which had however drunk freely. Two of the parasites (at 40 minutes) were dead spheres and two spent pigment. Crescents were plentiful in mosquitoes (1 in a field) the same day. Bugs tried—refused to bite.

. . . The Patiala affair was opposed by Government for a reason which I am not allowed to mention, but which in my opinion is bosh. The fact is that the Indian Government and especially the Medical Services are utterly ignorant of the importance of this kind of work. . . .

With kind regards,

Yours truly,

R. Ross.

Now, after failing to follow the alleged flagellated spores in the mosquito's stomach, I began to adopt quite another procedure. The insects were kept alive for several days after being fed and were then exhaustively searched for any parasites they might contain. If such parasites could be found, it would be possible that they might be developed from the motile filaments; and the point could be cleared up by subsequent experiments. I should have to feed mosquitoes on malaria cases as before, keep them for some days, and then examine them exhaustively for any parasites which might be present. On finding a parasite, I should have to work out its life-history and see if it occurred also in mosquitoes which had not been fed on cases of malaria. Of course this meant an immense study of mosquitoes—because, under the oil

[1] Afterwards developed into the "thick-film method" [72] (see page 475).

immersion lens, a mosquito appears as big as a hippopotamus is to the naked eye, and the object I was looking for need not necessarily be larger in comparison than, say, a nut or an apple, and, moreover, we had no clue at all as to the position the parasite would take up in the enormous mass of cells of which a mosquito is composed. Nevertheless I now began to attack this object in earnest. It was the method which solved the malaria problem, but it took me two years to find what I sought.

I had already found a parasite in the *Stegomyia* (probably *scutellaris*) mosquitoes which I had been working with and mentioned the fact in my letter of the 10 June (page 158). The objects which I had seen (see figure below) were little barley-shaped sporocysts, which were then often called *psorosperms* or *pseudonavicellæ*, and were known to contain the spores of the unicellular parasites called *Coccidia* and *Gregarines*. Accounts of them will be found in the textbooks of Leuckart and of Blanchard of that time. My next letters were full of descriptions of a series of specimens which I was sending to Dr. Manson, of various confirmatory observations, of failures to produce malaria in volunteers, etc. It is unnecessary to give this matter, and I therefore proceed with extracts regarding the psorosperms or pseudonavicellæ, the story of which I now worked out. My next letter describes some specimens which I am sending, especially some *Stegomyia* mosquitoes, with crowds of psorosperms in the intestine, and continues :

SECUNDERABAD, 5 *August* 1895.

DEAR DR. MANSON,

. . . Don't you think that these bodies must be looked to very carefully ? I have spent the whole week over them. It seems to me that they must still be watched even though they do turn out to be psorosperms. Leuckart and Blanchard say that the sickle-shaped bodies when freed from the psorosperm become amœboid (as for instance the intracorpusclar forms). The bodies appear to be very common among mosquitoes here. I remember finding however a much larger form about 20 in. in length with a little rosette near one end. I think that it was in a mosquito which had not been bitten; but I have not seen it again. Clearly by your theory it may be present in non-malarialised mosquitoes and yet be connected

with the malaria parasite. The bodies appear to me to belong to the sporozoa, and to be of a size which we may well imagine the flagella to grow into in a day or two. But you must not be surprised if I go on many a wild-goose-chase. They certainly appear " encysted and prepared for a lying in wait period."

Finding parasites in every case now, but no time to *work* at them. Have not a moment leisure from 6.30 a.m. to 8 p.m.! It is now 7 p.m. and barring half an hour for breakfast I have been at it since 8.0. Luckily feeling fit and full of energy. It takes three hours to hunt through a mosquito; but for my statistics I would throw over examining fresh cases of malaria any more for the present. . . .

With kind regards,

Yours truly,

R. Ross.

SECUNDERABAD, 12 *August* 1895.

DEAR DR. MANSON,

. . . I have to-day got to the bottom of the psorosperms —this *is* what they are. After writing to you I tackled a number of unfed mosquitoes finding psorosperms in many. I told you that they were in the intestine; but I find by a plate of insects' digestive apparatus in Blanchard that what I thought were the intestines are the Malpighian tubes, five of which in the mosquito are inserted into the *pylorus*. They are long blind tubes consisting of tesselated cells, full of black granules and easily seen as white threads by the naked eye, as of course you know; and they are present both in larvæ and pupa. I have now found a way of drawing out the whole apparatus with the ovaries or testicles away from the body cavity; so that I can make beautiful specimens, some of which I will send you.[1] Well, I found the psorosperms in numbers of unfed mosquitoes mostly at the closed ends of the Malpighian tubes where, if they are very numerous, they render the tube friable. I then began to study *grubs*, but found no psorosperms. This morning I took to the pupas, just before hatching into

[1] The method of dissecting mosquitoes now in general use. It was published in [32]. For Manson's comment, see page 189.

mosquitoes. In the stomach of the first were a number of gregarines of this shape:

about 100 to 150 μ long, moving rapidly just like slugs, thick end forward. They ceased to move in five minutes. They are full of dark brown granules, but I could'nt see the nucleus. Next I looked at the end of the Malpighian tubes—full of large oval bodies about 70 μ long, evidently *encysted gregarines*. On pressure, these and a number of encysting gregarines poured out of the ruptured tubes.

I saw no conjugation forms; the gregarines appear to be single. It was now clear by the *position* of these bodies at the end of the tubes that they were the parents of the psorosperms; but the next pupa put this beyond a doubt by *showing psorosperms inside the encysted gregarines*—so here is the whole chain at once. Now, what do you think? Is it still possible that these bodies are another form of the malarial parasite? Please let me have your opinion. So far as I have gone the gregarines live in the pupæ, the psorosperms in the mosquitoes. They seem to be hard encysted little bodies capable of enduring much rough usage, and may be discharged either in the fæces or when the insect dies and breaks up. Now the next stage is that of the falciform bodies, which, so far as I can gather from Leuckart and Blanchard, become *amœboid*. Is it possible that this amœboid *form is the intra corpuscular malaria parasite?*

I think, by your original article, that you have had this idea. It is clear in the first place that if the mosquito is the alternative host, the malaria parasite must be capable of living in it without man necessarily as an intermediary; we cannot conceive that the insect does no more than carry the parasite from an infected man to drinking water or mud; malaria must be a general endemic disease among mosquitoes; the germ must take only an excursion into men for change of scene and refreshment, so to speak. If this is the case then, we must expect the parasite to be a common one among mosquitoes and

capable of being propagated from mother to children ad infinitum; but also capable of rushing off into human beings, probably in its immature form. Such I read your theory to be; and such would probably be the case if the malaria parasite is the amœboid form of the mosquito's gregarine. These gregarines are evidently a very common disease among mosquitoes in this station; and so far as I can see, all the facts which I observe respecting the occurrence of malarial fever here can be explained *best* on the supposition that the poison is conveyed by mosquitoes into *isolated pots of drinking water* in the houses of the men. My reason for this is the following. The whole regiment lives in barracks or rather huts about ½ a mile from a large tank over which the wind blows towards us; the whole regiment drinks water from one large well. Now if the air or well-water were infected, we should expect nearly all susceptible people in the regiment to get the fever almost simultaneously, as happens in the case of cholera-poisoned wells. But what is the truth? The cases keep dropping in, one or two a day only, out of 700 men. Surely this suggests a most limited and isolated source for the poison; such in fact as the infection of a chatty or two of drinking water in the men's huts by ripe, infected mosquitoes. The mosquitoes themselves of course may come from the tank; and it is thus that lakes and marshes have got the reputation of being malarious; but, so far as I can see, the poison, however general it may be in a district, must be absorbed from very localised, and not general, sources, in order to explain the scanty and successive, and not general and simultaneous, occurrences of cases of fever (interesting errors!).

On the whole then, I think these gregarines fall in wonderfully well with your ideas of the alternative form. Even if they are not the malaria parasite, I can't help thinking that they will be found to have some human pathological import, because, as you say, men and mosquitoes fall exactly into the usual parasitological circle. The question is what do the free flagella develop into? Why not straight into young gregarines? I have not observed gregarines in malarialised mosquitoes, but I may easily have mistaken them for normal cells; because I would not have recognised them to-day, if I had not seen them crawling about, which they ceased to do in a few minutes.

Then again, the gregarines I have seen must have entered the young grubs, because they were fully developed in the pupa. Do you think that it is possible for them to enter the mature insect by way of human blood, and develop into psorosperms before its death ? Why not ? I will look very closely. I must also try to find gregarines in the grubs, and must see how the psorosperms leave the mosquito, whether by the intestine or only after death. I am certain too that I once saw a much larger kind of psorosperm, I believe in an unfed mosquito (don't remember).

You can imagine my excitement over all this—though it may turn out to be a wrong scent. My work is tremendous —I have *twenty* malaria cases in hospital—can't possibly work at them properly. . . .

13 August. Gregarines found in two more pupas and in a number of larvæ, down to small ones, but not down to the smallest size yet. Encysting gregarines in one old larva. They occur also in the intestine. Nuclei often clearly made out. By oil immersion the granules are seen in continual motion and a sort of bubble forms at the creature's big end [drawing]. Granules at tail bigger than the rest. Clear double contour. Length measured by micrometer 200 μ for about the largest size and 100 μ or less for the smallest. The youngest grubs contain the smallest gregarines. . . .

With kind regards,

Yours truly,

R. Ross.

SECUNDERABAD, 20 *August* 1895.

DEAR DR. MANSON,

. . . Having worked out the connection between the gregarines and psorosperms, the next thing to do was to find how the latter left the insect and how far they mature within it. Though gregarines are to be found in almost every grub, I have often failed in finding psorosperms in mature mosquitoes. Hence, if I had not overlooked them, I concluded they must pass out during life. I examined a number of unfed mosquitoes which had died naturally—no psorosperms ; then a number of fed mosquitoes at 2, 3, 4, and 5 days—only a few psorosperms occasionally. On the other hand they were often

plentiful in newly hatched mosquitoes; in one I found a single *gregarine*; in two others cysts full of psorosperms, hence it is probable that they pass out about the 2nd day after hatching; but how I cannot make out; I cannot find any in stomach, intestines or about the anus.

At the same time I examined water at the bottom of the bottle in which mosquitoes were kept—no psorosperms to be found. I looked at eggs which had been laid—same result. On considering large numbers of gregarines in grubs and pupas and often small numbers of psorosperms in even newly hatched mosquitoes I began to think that they were got rid of during pupa stage. I looked accordingly at the *cast off skin of pupa —numbers of psorosperms*. This fact is always confirmed by examinations up to day; so that this at least is one way in which the psorosperms pass out; though I can't imagine how they get from the blind end of the Malpighian tubes to the skin. They are most numerous generally in the head-piece of the pupa-pelt and are seen as numerous hard little bodies with a collection of granules at one part [drawing]. . . .

[21 August]. . . . Not a man doing microscopic malaria diagnosis in the whole presidency yet, so far as I know; the science not taught at Madras Medical Schools (of course every hospital assistant ought to be able to find the parasite); microscopes not supplied; no official circulars on the subject; no reply yet about your charts. . . . The fact is that by some fatuity the worst men rise to the top of the service here, that is the so-called smart men, the regulation-wallers, never the scientific men. Why was not V. Carter made Surgeon General? . . .

With kind regards believe me,

Yours truly,

R. Ross.

SECUNDERABAD, 31 *August* 1895.

DEAR DR. MANSON,

I was obliged to miss the last post owing to being suddenly engaged in a medico-legal case. It lasted from morning to night for three days; a hundred rupees a day, but forced me to waste some malarialised mosquitoes I wished to examine. . . .

As I told you the gregarines encyst in the pupa stage; and the psorosperms are mature and discharged *by the bowel* at the end of the pupa stage and the first days of the imago stage. I have now frequently seen psorosperms in stomach, intestine and about the anus. It appears that the gregarine cysts break up in position (that is in the ends of the Malpighian tubes), so that the psorosperms are passed out entirely free and are found free in large numbers in the Malpighian tubes themselves. The cast-off skins of the pupæ often contain numbers of psorosperms, as I said, but still numbers are carried away by the flying insect and evidently scattered abroad through its bowel as it flies. Thus I caught a mosquito which was trying to bite me; a stream of psorosperms was issuing from its anus and could be traced up the intestine, the stomach and the M. tubes. I have kept psorosperms in water for a fortnight now, but have not observed any change. The psorosperms themselves show certain differences which I will work at. They are seldom to be found I think in old insects either fed or unfed; and seem to be almost always got rid of before death. In order to find psorosperms in pupa-pelts it is necessary to separate the pupas from the larvæ, because the latter have the habit of sucking the cast-off skins, evidently for the numbers of infusoria and amœbæ, etc., which they contain; in this process the psorosperms are shaken out and sink to the bottom of the vessel, being *heavier than water*, or *are swallowed by the larvæ*. If however the pupa is put in a bottle by itself the skin is generally found full of psorosperms, and so is the digestive apparatus of the living insect which has emerged.

I think these are facts, as I have worked long and hard at the various points. The upshot is that the psorosperms must be discharged into two *different worlds* as it were; some are left in the water in which the grub lived and others are scattered—where? Now I will reason in the way you have taught me to. Suppose that the psorosperms were meant *only* to infect mosquito grubs, or perhaps some kind of fish or water-insect, would not the gregarines *make haste* to complete their development in time to allow of all the psorosperms being discharged into the water in which the grub lived; or would they not *retard* their development in order to allow the discharge to take place when the *eggs are laid*? But as a

fact most of them are got rid of just at a time when the insect is *farthest* from water and is busy *hovering round men*. It is indeed almost certain that *while the insect is biting* it is discharging psorosperms upon the human skin, which may be directly conveyed to the mouth by the man scratching himself and then accidentally sucking his finger. How does this look ? Or the psorosperms may be simply scattered over a sleeper's mouth.

I must say that this *psorosperm* theory is looking very promising, especially this latest development of it, which I have been driven to by hard facts. Nature would I apprehend scarcely wasté the psorosperms by causing them to be evacuated just at the moment when the insect is farthest from the water—that is supposing they are meant only for grubs or water animals. In short, I believe that the psorosperms are *meant* for *men*, as well as for reinfecting the grubs, of course ; hence their being discharged into *two worlds*. But what disease can they possibly cause ; not dysentery I suppose ; malaria seems the only supposition. Well, I have got three tubes containing psorosperms, and I hope to give each of these tubes to a man tomorrow, but I must begin with small doses of psorosperms—so you must not be disappointed if I fail at first.

1st September Sunday. The contents of three tubes given to three healthy young men at 8.45 this morning. Each tube contained psorosperms from the insects (direct from the Malpighian tubes). One contained a pupa-pelt. All given in water by mouth. If the experiments fail I will try spraying the psorosperms into the lungs. . . .

The nature of the experiment was fully explained in the presence of witnesses to all the men who submitted to it.

I have a sort of feeling that it will be successful, and, as you will readily understand, a kind of religious excitement over it. God knows what will be the result. If the experiment fails I shall have to take a holiday, because the work and excitement are telling upon me ; but I shall soon begin work again on other lines. I feel convinced it is the mosquito. . . .

3rd. Holiday to-day (only the sick of 3 regiments, one court marshall [sic] and a medical board). . . . In this station there are about a dozen doctors, not one of whom possesses an oil immersion or works at malaria. Is this to go on for

ever? Yet Laveran's discovery is essential to treatment. I have just been adding up my results for four months. I have recorded 110 cases (ague or simple continued fever). Out of these I have found the parasite in 65 or 59%, having missed it in numbers of other cases simply owing to pressure of work, I expect. . . . Now is not the apathy of the heads of the department quite monstrous? It amounts to a national scandal. Why don't they supply proper microscopes and make their men work at the subject? Why don't they teach it in medical schools? In districts where fever is prevalent why don't they have the fever investigated by the miscroscope? Really if nothing is done soon there will be a scandal. The fact is that our surgeons general don't seem to recognise their positions or their duties in respect to new scientific discoveries. It is the same with amœba coli. . . . Moreover qualified men should be sent round the country to teach—Laveranity apostles. Oil immersions ought to be supplied at once. . . . I have examined the stomach, intestine, kidneys and ovaries of three malarialised mosquitoes pretty carefully, but have seen nothing which I can swear are parasites. They were killed at 3 and 4 days. If the psorosperms fail I must of course work entirely in this direction. But except the psorosperms I have seen nothing yet which looks sufficiently hard and encysted to withstand *out-of-door* life. The hitch in the psorosperm theory comes in when we enquire what the flagella develop into. Is it possible too that a single animal can undergo so many metamorphoses? I am looking forward anxiously to your opinion of it all.

With kind regards,

Yours truly,

R. Ross.

I have given my work and thoughts on this gregarine parasite of the mosquito at some length because of its interest. In our stage of knowledge at that time, my conjectures that it might be the mosquito-stage of the malaria parasite were quite reasonable, and the matter had to be worked out experimentally down to the fullest detail. It is the method of "trial by error" which is often the only method that can lead to success in the solution of many problems, including mathematical ones; and it will be seen later that I had to pursue

the same method for every organism which I found in the insects. Really the great objection to the gregarine theory was contained in the penultimate sentence of the last letter. I had found the complete cycle of the gregarines, and there was no room left, according to general parasitological knowledge, for another cycle consisting of the malaria parasite, to be interpolated.

But I now perceived that my hypothesis was beginning to run away with me a little, and that I must take time to give my mind a rest. I therefore intended to attempt to follow the flagellated spores again as suggested at the end of my letter of 18 July. For this purpose I proposed to mix the blood contained in the mosquito's stomach with a little salt solution, with or without stain, so that the whole contents of the said blood might be spread out evenly under the microscope, enabling me to follow the flagella exactly. If I had done this I would have anticipated Macallum and Opie in their fine discovery made two years later, and would no doubt have actually seen that the flagella are sperms. In fact I came very near this discovery in my letter of 18 July, describing the " adventures " of one. But unfortunately, just after writing the letter last recorded I received orders to proceed forthwith to Bangalore.

During this period other things were done. Being a convert, like St. Paul, I became a militant apostle. Surgeon-Colonel Lawrie was then the head of the Medical Services of the Nyzam of Haidarabad, and lived at Chudderghat, near Secunderabad. He was a very capable man, a brilliant surgeon, and the organiser of one of the finest medical schools in India—direct, energetic, and witty. But he did not believe in " Laveranity " because, in fact, one of his officers had made the mistake which I had previously alluded to of taking artifacts for malaria parasites. I gathered that he detected the error, and, like myself, therefore disbelieved in the whole business. But he carried his scepticism too far and did not know enough about the blood to recognise that the real Laveran bodies were indeed parasites. I had an amusing controversy with him in a paper which I published later [22], and a still more amusing interview, at which I suffered a crushing defeat (page 215) !

CHAPTER XI

THE SCAVENGERS. BANGALORE, 1895–1897

My transfer to Bangalore on special sanitary duty was the first of the three serious interruptions from which my malaria work suffered, all of which by some evil fatality occurred just at the moments when rapid advances seemed to be assured. But unlike the two later interruptions, this first one was justified by the event because it gave me an almost unique experience in practical sanitation which was invaluable when we came after 1898 to apply the proved mosquito-malaria theory for the benefit of suffering humanity.

As stated on page 57, the civil and military station of Bangalore is an island of territory ruled directly by the Government of India within the dominions of the Maharaja of Mysore. It was under the jurisdiction of the British Resident at the court of the Maharaja, and contained (1) the military garrison, under the General Commanding, and (2) the civil population of about 90,000 people, under the Bangalore Municipal Commission. On previous occasions my post of Staff Surgeon at Bangalore was connected with the garrison; but I was now sent for by the Resident (then Mr. Lee Warner, Indian Civil Service) in connection with the affairs of the civil population and the Municipality. The occasion was this. For a long time that capable officer, Surgeon-Lieut.-Colonel A. F. Dobson, M.B., Indian Medical Service, who was then Residency Surgeon (that is, the medical officer to the Resident, the civil staff, and the Bowring Civil Hospital) had been much dissatisfied with the sanitary administration of the Municipality. The native quarter was in a filthy condition and was frequently swept by epidemics, especially of cholera. Now in August 1895 another outbreak of cholera was beginning, and he considered that the Municipality was not working hard enough or wisely enough to check it. There was a small Sanitary Board, consisting of the principal medical officer of the troops, Surgeon-Colonel S. B. Hunt, Army Medical Service, the President, Dr. Dobson, the Secretary, and a junior military officer;

and this Board complained to the Resident, especially in a letter No. 651 of 11 September, and advised a complete reorganisation of the sanitary affairs of the Municipality, then consisting of about twenty elected native members and six *ex-officio* members (mostly British), under an Indian civilian as chairman (then Mr. A. M. Slight—an excellent young man). But as no reform was likely to occur within this municipality itself, Dobson advised Mr. Lee Warner to ask the Government of India for my services, partly to deal with the cholera and partly to report on the sanitary condition of the station in general. It was rather a miracle, I think, that this request was acceded to, even though I held a diploma of Public Health ; but I received my orders, closed the malaria investigation for a time, packed up my few belongings, and arrived in Bangalore on 9 September 1895.

On my arrival I received the most astonishingly ample and indeed dictatorial powers. These were : " To direct and enforce the sanitary operations conducted by the Municipality in consultation with the Residency Surgeon ; to submit a report to the Resident on the present state of affairs. . . . To examine thoroughly the constitution of the Sanitary Department and to suggest in full detail the changes needed to improve its efficiency . . . . To deal specially in my report with the following matters, etc." Seldom has a mere doctor been given such powers, and I availed myself of them. Taking up my residence in a tent in the compound of the West End Hotel, I was given a carriage for my use day and night, a clerk, sanitary inspectors, and peons. In two days, in consultation with Dobson, I sent the Municipality an ultimatum regarding measures which they were neglecting, and followed this by numerous similar documents ; while every hour I rushed off to see the cases of cholera scattered over the station. Within five weeks I sent the Resident six emergency reports and extracted 5,000 rupees from the Municipality for the necessary work ! There were 20 deaths from cholera in August, 64 in September, 7 in October ; and then the epidemic ceased and I set about the general sanitary investigation. My full report to the Resident (Residency Press, Bangalore) was submitted on 20 January 1896, and contains 126 printed foolscap pages dealing with the whole subject (and is, I am told, still in use).

From 18 January 1896 I was appointed, professedly under the Municipality but really under the Resident, to carry out the difficult task of giving effect to my own recommendations, and on 26 May 1896 I became Officiating Residency Surgeon,

while Dr. Dobson went home on furlough. On 29 March 1897 I took leave to Ootacamund, pending Dobson's return from furlough, thus ending my arduous but agreeable duties at Bangalore. I had been there for eighteen months. Soon after I arrived, Mr. Lee Warner had been succeeded at the Residency by Mr. Mackworth Young, I.C.S., who, in turn, was followed by Colonel D. Robertson. After I left Bangalore, the last-named published the following order, dated 7 April 1897—of which I am very proud: "Surgeon Major Ross," he said, "having left Bangalore, the Resident desires to place on record his high appreciation of the services rendered by this officer. . . . Surgeon Major Ross's interesting and exhaustive report on Cholera, General Sanitation, and the Sanitary Department and Regulations in the Civil and Military Station of Bangalore was received in January 1896, and in reviewing it, Mr. Mackworth Young, the late Resident, remarked that Dr. Ross had 'discharged the duty entrusted to him in a most able and efficient manner,' and he requested that his best thanks might be conveyed 'for his excellent report, which will contribute largely to the welfare of the Station.' The main principles of that report were accepted by the Municipal Commissioners, who were so assured of the soundness of the advice offered by Surgeon Major Ross that they applied for his services in order that he might initiate the reforms advocated by him and set in order their Sanitary Department. In the execution of his various and important duties, Surgeon Major Ross has never spared himself. His work throughout, in which a large measure of success has been attained, has been remarkable for the zeal, thoroughness and tact displayed, and the Resident is confident that for this work no more capable officer could have been selected." Copies of this were sent to the Foreign Department of the Government of India and to the Surgeon-General with the Government of Madras. But I have never been employed again by my countrymen on this kind of service.

I have no space for details. We had to revise the whole system of conservancy and refuse-removal, the latrine system, the surface drainage, and even taxation and the Municipal Regulations and Bye-laws—to institute a municipal laboratory, registration of vendors of food, milk, and water, and a hundred other improvements. From January 1896 I was an *ex-officio* member of the Municipality and, with the continual and effective support of Surgeon-Colonel Dobson, carried through measure after measure, often against strong opposition.

The list comprises no less than fifty-eight proposals, resolutions, or rejections! Since then I have always been very sceptical regarding the worth of all councils, committees, and public assemblies. Most of the members, both European and native, were "guinea-pigs"—that is, persons who knew nothing of the business on hand and were too lazy to learn, and who, when they were required to vote, would look furtively round the tables to see how the majority were likely to vote; and their opinion was absolutely worthless. A few were practising oratory at the expense of the others; and some were trying to be professional politicians—that is, persons who cared nothing for the rightness of a policy but everything for the effect of their speeches when reported in the "local press." I tried to persuade these humbugs to go round their wards with me in order to see the dirt, squalor, and ill-health of the people—they were, of course, always so sorry that they were otherwise engaged. The leader of the "opposition" was a retired English engineer, who was very angry because he was not allowed to put his finger in the sanitary pie; and the measure of ours which caused the most "popular indignation" was my cess-tax, a small payment for the daily removal of ordure, which the ingenuous householders used previously to keep for weeks in their houses in order to sell it to contractors for manure. The papers also attacked me for this "imposition," and I received abusive anonymous letters about it. One said, "Have you no sense you fool? Shame, shame, shame!" On the other hand several of the Commissioners were admirable persons, especially Mr. Abdur Rahman (I think), a magistrate, and Mr. Annasawawmy Mudaliar, who was not only as good a speaker (in English) as I have ever heard, but who often accompanied me in my rounds, studied my proposals, and persuaded his poor people to adopt them—a Hindu who possessed every virtue of a Christian gentleman. I also received much help from Captain A. C. Joly de Lotbinière, R.E., who was engaged in providing a piped water-supply for the town; and Mr. Slight, the president, though he was criticised by Dobson, always tried to help us. On the whole we had nothing to complain about, for we passed almost every measure we wanted, and I am sure that all my "enemies" were also my friends.

The street gutters of the native quarter were really shocking, as they ran with sewage; and I therefore devised some new patterns and described them in a *Memorandum on Open Sewage Drains*, which was published by the Municipality on 24 Sep-

tember 1896. They were tried in a slum in South Blackpully, and it was interesting to watch how the whole quarter was improved by this means—the householders mended their walls, doors, and yards in emulation.

Our sanitary reforms were not likely to be immediately effective, and we had three outbreaks of cholera during 1916. As usual, the disease began to creep up from the east coast in April; and I posted hospital assistants at railway stations and cross-roads to intercept cases, and disinfected wells and instituted a system of rewards to our sweepers and scavengers and others for reporting cases (the politicians of the Municipality accused me of paying spies to watch the householders !). On 4 June a case got through into the town, and on the 14th and subsequently twenty-seven cases were reported close together. I saw by my "spot-map" that they were all probably infected from the same public well and closed it on the 14th. The epidemic ceased by 20 June. On 15 July, however, it commenced again in another part of the town, but was again suppressed there after a fortnight by active disinfection of all the wells and closing of the most suspected ones. We had scarcely begun to congratulate ourselves on this result when, on 8 August, a terrible epidemic began in the worst parts of the town called North Blackpully and the East General Bazaar—areas swarming with slums and containing about 20,000 of the poorest people. There were 582 wells here within a space of about one-quarter of a mile square, most of them within private tenements where we could not easily touch them owing to "caste prejudices"—that is gross superstitions. We did what we could. We closed the wells in batches; we disinfected them over and over again; we told the people to boil their drinking water and provided it ourselves; we gave them hot coffee and medicine early in the morning; we disinfected backyards, drains, and rooms occupied by the dying. All in vain. The angel of Death had descended amongst them and smote the poor wretches right and left. They died within twelve hours. Secondary cases were numerous: where the child died to-day, the mother or father would be dead to-morrow. The people, usually so patient and good, were stricken with terror, cried to their gods, formed processions, and glared at those who tried to help them; and the heavy rain washed the reeking filth of the streets and latrines into the shallow poison-pools they called wells. It was a dreadful time. We could do nothing more. Either the cholera vibrio had got into some source of water which we failed to find and

close, or it had penetrated into the whole sub-soil water of the locality. Then suddenly, as usual after about a month, the pestilence cleared and vanished like an evil thunder-storm. But 219 cases were reported during that time, of which about 150 died; and probably very many of the cases and deaths were not reported at all in the final rush and terror.

I should like to take the politicians who so often belittle and neglect sanitation and medical science—I should like to take them by the scruff of the neck and fling them into the midst of an Indian outbreak of cholera, and into the midst of the filth and dirt which their bad laws and feeble administration allow to continue! I am not talking of British politicians only, but of Indian politicians, such as Municipal Commissioners, as well—they are all alike. In this case they had allowed these slums to grow up during years of neglect; every house had its poison-pool, and science could do nothing.

Yet sixteen years had elapsed since Robert Koch had discovered the cause of cholera. Unfortunately certain "eminent scientists" would not believe him, and one of these was the official investigator of the Indian Government. The world still fails to recognise that Tom, Dick, and Harry are not necessarily good scientific investigators. Often they do not possess the brains either to make sound discoveries themselves or even to understand them when made by others. They can pass examinations, write textbooks, and become "eminent scientists," it is true; but the real investigator, like the real poet, is born not made. Now governments love to appoint Tom, Dick, and Harry to their most important scientific posts, and then look upon them as being true prophets. One might as well try to establish a college of poets out of a band of literary journalistic critics; and thus it was that India neglected Koch's discovery all those years. Now however, in 1896 a revolution was beginning: Koch's work had been proved completely, and Mr. E. H. Hankin, Chemical Examiner at Agra, had been following it up by finding the vibrio in cholera-infected food and water, and by showing how to disinfect wells by permanganate of potash. Also Mr. W. M. W. Haffkine had arrived in India (in 1893) from the Pasteur Institute, bringing with him his famous Anti-cholera Vaccine, and was now trying it in India on a large scale—and he laughed at the hypercritical criticism of the Indian authorities. Thus things were now beginning to move in India at last. But while I was able to employ Hankin's disinfection of wells at Bangalore, I was not able to use Haffkine's vaccine because

my laboratory and staff were not nearly developed enough to give it to large numbers of people, while the people themselves would have objected in those days. During the sixteen years' neglect of Koch's discovery about eight million Indians had died of cholera; and I may add that there was enough *prima facie* evidence of the discovery to have justified extensive work on the subject from 1880 onwards.

These epidemics were reported by me to the Resident in two letters dated 8 July and 29 September 1896, and I was much distressed at our failure to check the last outbreak. The following rough verses occur in one of my *In Exile* notebooks:

Twice have I driven thee hence,
Defeated, dreadful Guest—
O murderous Pestilence:
This time thou conquerest.

Loudly the people's cry
To thee in prayer swells;
I seek to purify
The deadly poisoned wells—

In vain. The languid child
Lies on his mother's knee;
The mother follows; wild
The people shriek to thee.

On one occasion I tracked a source of infection to a well-to-do milkman, who had kept the best and cleanest cow-house in the station. I charged him with having a case of cholera. He denied it; but just as I was leaving I saw a movement under some clothing cast in a corner. It was his little daughter dying of cholera.

But the abiding questions were those of general sanitation. On 17 February 1896 I went to Poona to inspect the admirable system of poudrette manufacture which had been installed there for years by a most energetic and capable native member of the Municipality; and later we adopted his method, with some simplifications, at Bangalore. Thence I went on to Bombay to study many sanitary details there. It was just before the terrible Indian outbreak of plague had commenced in the city, though even then there were rumours of it; and the case was another instance of want of scientific study and preparation by the Indian authorities. On the way back I spent a day (23 February) at Khandala, on the Ghats, in order to see the great caves and their carvings.

Above all it was necessary to organise refuse-removal on the cheapest and most efficient basis possible (there were no house-sewers), and day after day I accompanied the scavengers and sweepers in their early and disgusting work—sometimes rising before daybreak to do so—in order to learn exactly how an Indian city was to be kept clean and what improvements should be made. These experiences are not easily forgotten—the glorious stars glittering in the chill morning wind, the murky lanterns, the clinking pails, the patient oxen, the awful stenches. And the poor men themselves, the last pariahs and outcasts of society, toiling while others slept; and yet, in a way, the civilisation of the thronged cities was based upon their labour. Thank Heaven I was able to increase their pay and give them good lanterns, at least.

Great is Sanitation—the greatest work, except discovery, I think, that a man can do. Here is a city seething with filth and disease. What is the use of preaching high moralities, philosophies, policies, and arts to people who dwell in these appalling slums—sometimes whole families of them crowded into one cell, mixed with cattle, vermin, and ordure? Your job, Sanitarian, is plain! You must wipe away those slums, that filth, these diseases. You shall work in the darkness while others sleep. None shall know of your labour, no one shall thank you, you shall die forgotten. The great ones of the earth shall despise you, shall hamper you, shall even punish you. The lofty rulers of the world shall not deign even to look at you; but shall prate of gods and virtues, liberties and laws, and shall busy themselves pouring the wine of wealth from one vessel into another, drinking much by the way, and spilling more! But you, O Cleanser, shall always be a Pariah. Fret not, however; for these dying children shall live, and some day this hideous slum shall become a city of gardens, and it is you who will have done it.

We shall reach the higher civilisation, not by any of the politicians' shibboleths, methods of government, manners of voting, liberty, self-determination, and the rest, all of which have failed—but first by the scientific ordering of cities until they are fit for men of the higher civilisation to dwell in. We must begin by being Cleansers. I find the following two stanzas in my notebooks:

> "Ascend," the Prelate cries,
> "From Men to Angels." "Then,
> First learn," the Sage replies,
> "To grow from Apes to Men";

and also

> We cry, "God, make us Kings,
> Poets or Prophets here!"
> The scornful Answer rings,
> "First be My Scavenger."

Of course, owing to the great pressure of my sanitary work, I was unable to continue my malaria researches for months after my arrival in Bangalore in 1895. But I continued to write letters to Manson from time to time. My first one from Bangalore was dated 18 September, and in this I told him the results of my experiments in which I gave psorosperms to three young men. All of them had slight reactions, and I wrote: "The two cases were like Lutchman's and I think that the psorosperms bring on some reaction, *probably malarial.* The cases struck me as malarial, due to *insufficient* doses. Will try 3,000 psorosperms next time . . . a fresh cholera case and must be off at once . . . have plenary powers to reform all sanitary deatils." In his reply of 9 September, Manson was very doubtful about the theory that psorosperms are connected with malaria, but added, "Still it is necessary to clear the ground of gregarina culicidis and you do right to get clear conceptions on this point before going further." I wrote to him on 4 October: "I am of course not sure as to the result of my experiments of the 1st Sept. but two of the men certainly got something like malarial fever of a very slight kind; my only conclusion is that psorosperms cause some kind of reaction, query what kind? Further experiment however may still prove me to be mistaken. Now about some points of your objections." I told him that I had failed to infect young mosquito larvæ which had lived in water full of psorosperms, and doubted whether the whole history between the psorosperm and the young gregarine was fully known yet. On the other hand I thought "It would indeed be very curious if the same parasite were capable of going through two separate life-cycles. . . . I admit all the difficulties, especially about the flagella, and don't presume even to put forward a gregarine theory; but it is curious that (as I believe I haven't told you) I could find gregarines only in the mosquitoes at Begumpett (my regiment's quarters) the most feverish locality in Secunderabad. Just before leaving Secunderabad for this place I examined five or six batches of grubs from other parts—no gregarines at all! Then again after I suppressed the gregarine bearing mosquitoes at Begumpett on the 22nd August, there appears to have been scarcely any more fever in the regiment.

. . . 8th October. The difficulty, if the malarial parasite in the mosquito is like what it is in man, will lie in distinguishing it from the nuclei of cells ; I have seen so many things like malaria parasites in the mosquito and in the grub ; but I have a lot to learn about their anatomy." I then gave Manson a summary of the cases of malaria in which I had found parasites in Secunderabad. They were : " 112 fever cases ; 21spring tertians ; 38 summer-autumns ; 2 quartans ; 3 mixed ; 5 doubtful ; 69 total parasites found." In the rest of my letter I described my work in Bangalore and remarked, " I tell you privately I have been credited, perhaps by a silly slip of the pen, with almost unlimited power ; and I have seized upon the order (or mistake) and immediately *commanded* the Municipality to make three great reforms. Dobson is at my back. . . . I have acted with such celerity that the astonished President of the Municipality . . . has conceded my provisional demands and has allowed me actually to carry my reforms half through before he or the Resident or anyone understands the tyrannical nature of my action."

Shortly after arrival in Bangalore in September 1895, I found that my wife could join me there ; and she arrived in Madras about 23 November with our three children and their nurse, Miss Agnes Carrington. On 26 November 1895 I wrote to Manson : " I went down to Madras however to meet my wife who has arrived with the children from England, and I took the opportunity of showing them all (i.e. the doctors) the malaria parasite and now they are at it keenly, especially a man called Sturmer who writes me that he has found the parasite in five out of nine cases already. I showed them a triple quartan and a summer-autumn, the latter occurring in a case of elephantiasis which a man showed me in order to catch me, thinking that he had elephantoid fever. The laugh was turned against him . . . my labour has been frightful—working from 6.30 a.m. to 11.30 and 12 at night. . . . I have not been able to find any psorosperms here or in Madras. . . . I enclose a letter from Sturmer to me which will show you how keen he is. . . . None of them in Madras were able to find the animal till I went down there. But I saw Cleghorn [the Director-General, Indian Medical Service] in Madras and spoke about malaria. Everything is now merely a question of want of time with me ; there are only twenty-four hours in a day. I reported about mosquito investigations to Government in response to a circular which Cleghorn sent round."

Manson's replies deal chiefly with his studies of the specimens

which I had sent to him, and with requests for more specimens. He did not seem to be much interested in Bangalore sanitation! With reference to my method of finding crescents (page 167), he remarked in his of 9 September: "I like your dodge of finding crescents and shall try it in the first case I get. It ought to answer with other pigmented forms as well and should prove a great gain in diagnosis. If it comes out all right I shall send the part of your letter referring to this to the B.M.J. adding that I have tried it, and find it good." On 21 October he wrote to me regarding my method of dissecting mosquitoes (page 170): "How do you manage to get the alimentary canal out so perfectly without injury to malpighian tubes and other delicate structures?" Both of us were much alive to the possibility of other workers stepping in very soon to complete my interrupted work on the mosquito theory; and indeed on 26 July 1895 Manson had already written about the theory, "The Frenchies and Italians will pooh pooh it at first, then adopt it, and then claim it as their own. See if they don't. But push on with it and don't let them forestall you. They won't have time this autumn, and they will not have a chance to work seriously at the matter till next June or so. You have got a year ahead of them and in that time you should have solved the problem in its principles at all events. Leave the details for them to work out, which they will do well and with much wrangling." As a matter of fact they did not begin on the work until *three* years later, that is in the summer of 1898, after I had solved the problem; and the difficulties were much greater than Manson supposed.

Even in the midst of my very hard sanitary work I found time to search for malaria cases at the Bowring Civil Hospital. On 4 December 1895 I wrote to Manson saying that I had a good case of crescents and was trying mosquitoes again; and I then added that I was "inclined to claim discovery of a new species of malaria parasite namely the eye-forms (page 165). Found two cases, of dysentery and fever, the other fever, with numbers of these. They are certainly living creatures and are motile to a small extent but not amœboid. Are they young crescents?" I think this was nearly the last time I was deceived by these bodies, as some months afterwards I found numbers of them in my own blood, and in that of other healthy persons. They deluded numerous observers, and I remember that one writer claimed them as the cause of fever in Palestine. They are probably micrococci from the skin which fall into the red corpuscles when the specimen is made.

In my letter to Manson of 12 December I apologised for not sending all the specimens which he had asked for so frequently. He wanted mosquitoes fed on malaria cases and preserved in glycerine or formaline; but, already overworked as I was, it was a very serious matter for me to obtain them, because I had first to hunt for malaria cases through the station, and then to try and get the mosquitoes to bite. At the same time I was sure that he would not be able to get good results from sectioned mosquitoes, "for the simple mathematical reason namely that unless the blood is swarming with parasites you won't find more than one or two in a whole section." I always felt also, though I did not like to say so, that he could have done the work in London almost as easily as I had done it in Secunderabad. I was wasting what little time I had to spare.

My letters of 29 January, 26 February, 25 March, 5 April, 26 April, and 27 May 1896 record only want of time to do much malaria work; but during that period I tried to infect some more men with psorosperms, giving nineteen experiments altogether. What really made me sceptical was a fact noted in mine of 25 March: "By the by I have entirely forgotten to tell you up to date that I myself took some psorosperms in October and again in December—no result at all. I took about 1,500 and 2,000 on the two occasions; but then I have never had fever though I have been in malarious places. Three days after the second dose I thought I felt ill but it was probably liver." And when such cases failed entirely I thought that I had done enough in this line and began to consider whether some other route of infection was not possible. Thus on 27 May I wrote to Manson: "None of the experimental cases have come in yet and all look like being failures. The last case appeared to be looking ill after 48 hours, but it passed off next day and he has been well ever since. The psorosperms were given at all degrees of freshness and in large numbers so it certainly looks, as it was to be suspected, that they are not the thing. But I will watch and pray, and try a few more experiments; . . . but the belief is growing on me that the disease is communicated by the bite of the mosquito. What do you think? She always injects a small quantity of fluid with her bite—what if the parasites get into the system in this manner. I shall experiment in this direction and shall also dissect the head." *This in fact proved to be the case.* See Manson's opinion, page 193.

During this period, however, I tried to push the study o

the parasites and told Manson on 26 February: "I preached Laveranity at Poonah and Bombay, making one convert in the former. . . a young chap called Kilkelly, I.M.S.; saw the Parsee doctor Surveyor, whom you knew in Bombay, and showed him a spring tertian; and also saw some relapsing fever, 'spirilla.' Do you know I believe it is an *animal parasite* some possible connection with surra (?)—hardly possible, but it does not look like bacteria. The Bombay men don't care a rush for the malaria parasite; they won't take the trouble to look at my spring tertian. Madras men are waking up well." In fact Dr. Sturmer, I.M.S., whom I have mentioned before and who was one of the district medical officers in Madras, came to Bangalore during Christmas 1895 to work with me; and in January I made many demonstrations to Captain Carr White, I.M.S., who had succeeded me in the Staff Surgeoncy at Bangalore, and who wrote a paper confirming my results in *The Indian Medical Gazette*, April 1896.

All this time we had been living at the West End Hotel, but in the hot weather I sent my family to the Farringdon Hotel, Ootacamund, and myself took a few days leave there—which was all the leave I had during that year. On a second occasion I was called back prematurely by the outbreak of cholera already described. When the rains began my family returned to Bangalore, and we all lived in Surgeon-Colonel Dobson's house on the high ground, namely, 4 Cathcart Road—he and Mrs. Dobson having gone to England on furlough; and we stayed there till I left Bangalore finally in 1897.

I was not able to do much malaria work until September 1896, but, as Dr. Dobson had left for home, I was then in charge of the Bowring Civil Hospital. I began at once by testing experimentally the hypothesis which I mentioned to Manson in my letter of 27 May, namely, that malaria may be communicated by the bites of mosquitoes and not, as Manson thought, by people swallowing water in which infected mosquitoes had died.

A number of mosquitoes, all bred from larvæ in captivity, and of all the kinds which I could collect (many specimens of brindled and grey mosquitoes), were fed upon several patients with numerous parasites in their blood. One of these patients had all three kinds of parasites in him; and I specially employed this case, as well as many varieties of mosquitoes, in order to increase the chances of one at least of the species of mosquitoes present being appropriate for one at least of the species of parasites. After feeding, the insects were kept alive for one

or two days, and were then applied in considerable numbers on two occasions to Mr. K. N. Appia, Assistant Surgeon of the Bowring Civil Hospital at Bangalore, who courageously volunteered for the experiment. Mr. Appia had suffered from malarial fever some years previously, but not since then; so that if he should be attacked by fever shortly after the experiment, it would be strong evidence, if not proof, in favour of the inoculation theory. He remained, however, absolutely free from fever. He was then bitten by five mosquitoes, which had been partially fed *immediately before* on a case of crescents—on the supposition that the insects may carry the infection mechanically, as the tsetse-fly carries nagana; but the result was again negative. Lastly two other individuals were bitten by mosquitoes fed from three to five days previously; still without effect. I judged then, either that infection is not produced in this way, or that the proper species of mosquitoes had not been employed, or that they had not been kept for the proper period after feeding; and I proposed to return to the subject again.

These experiments were reported to Manson in my letter to him of 21 September 1896, and full descriptions of them, and of the previous experiments on men with water containing dead mosquitoes or psorosperms were published in December 1896 [23]. My ultimate conclusion was given in this paper as follows: "On the whole then, I think, I am justified in saying this much: that, while we cannot dream of stating definitely on the strength of these experiments that there is something connected with the mosquitoes which is capable of imparting fever, the three positive results are still curious and tend to be in favour of the truth of Manson's theory." Of course I had to give considerably increased rewards if the subjects of the experiments got any fever; and many of us thought and think that some Indians are clever enough to get fever whenever they like; and my ultimate conclusion was that the apparently successful cases which I had obtained were probably of this nature. Anyway the facts stand as I stated them. Perhaps also the psorosperms do really cause some reaction in human beings, although we now know that they are not in any way connected with malaria.

All this time Manson had been disturbed at what he evidently thought were my sanitary divagations, and on 20 May 1896 had written: "It seems to me a great misfortune that the best men in your service are the hardest worked, that is to say, that their energies and abilities are directed into such base

utilitarian channels as practical sanitation of cities and the charge of big establishments requiring more administrative ability than brains." I did not agree with this view at all. To me it always seemed that the practical application is the summit of all biological research. I did not undertake this work on malaria in the interests of zoology, but in the interests of practical sanitation.

In his next letter of 12 October Manson commented as follows on my experiments made to try to infect healthy persons by the bites of mosquitoes, and remarked: "Your experiments on mosquito biting are interesting although negative. It may be that the mosquito conveys the parasite in biting but I do not think so—at all events I do not think that it does so directly; for the habit of the mosquito is to bite once only, and the normal evolution of the parasite is sure to correspond and to run parallel with the normal habits of the insect. I think you are more likely—assuming that the parasite is conveyed by the bite of the mosquito—to find that it is conveyed by the offspring of the mosquito that has bitten the malariated individual—that it is conveyed by the children and not by the parent seeing that on the flagellum hypothesis it has to evolute in the insect that has bitten and swallowed. You might try feeding mosquitoes on a malariated man, keeping them, rearing their young and setting these to bite an uninfected subject." As a matter of fact, this story of a carrying agent communicating a disease to its progeny and thence to the original host was simply the story of the *Piroplasma bigeminum* of oxen, which is carried by ticks in this manner, as previously proved by Smith and Kilborne (page 123). I did not know this at the time, and if Manson knew it he did not mention the fact to me. I proposed to do some work on these lines, but am glad that I did not waste time over them.

In March 1896 Manson had developed the whole of his theory in the three Goulstonian Lectures delivered before the College of Physicians in London [20]. He argued the case admirably, and reiterated his view that the so-called flagellum is really a flagellated spore which escapes in the stomach cavity of the mosquito in order to infect it. In the second lecture he showed the analogy which exists between his hypothesis and the general principle of metaxeny of parasites, especially in the case of the filaria, partially discovered by him, and then gave his hearers in brief much of what I had written to him, such as the adventures of a flagellum (page 166), and the life-

history of the mosquito gregarine (page 170). His third lecture was devoted to an exposition of my work during 1895, describing especially the "ex-flagellation" of crescents in mosquitoes, and my conclusions given in my letter of 28 May. And he said that "we may conclude with confidence that Ross has thoroughly proved by direct observation my hypothetical conjectures that the stomach of the mosquito is a suitable medium for the flagellated phase of the plasmodium malariæ to develop in." He concluded by countering some possible objections to the theory. The whole lecture should be studied by those who are interested in the development of scientific hypothesis.

But now in his letter of 12 October he sent me something of a bomb-shell. A. Bignami, a Roman physician, had done much admirable work on malaria, especially in conjunction with E. Marchiafava, and had considerably extended the work of Laveran and Golgi in detail. His work and that of other Roman writers was brilliant, but showed some undue tendency to absorb the credit of previous writers, and he was indeed one of the triumvirate who attempted to pirate my work in 1898; but he now published an article in the *Policlinico* of 5 July 1896 [21], objecting to Manson's mosquito theory and attempting to substitute one of his own. The objection was based upon what was really a mere speculation, that the flagellate bodies are not living creatures but are dying organisms—that the appearances due to the escaping flagella are really nothing but the dying struggles of these bodies. So far as I can make out, this hypothesis had been started some years previously by another member of the triumvirate, namely B. Grassi. The only reason for the speculation was that no one had then succeeded in finding chromatin in the flagella; and when Bignami wrote his objections to Manson's theory he did not know that in fact the chromatin had just been found in the flagella by Sakharoff [15]. Indeed Manson had already anticipated this difficulty in his Goulstonian Lectures; but as few people knew the facts, and as Bignami was a man of considerable eminence, the Indian authorities immediately began to think that Manson and I had been quite mistaken; and it was necessary for me to disprove Bignami's contention. Manson had sent me a translation of Bignami's paper [21]; and I thought that the paper was trying not only to discredit Manson's hypothesis but also to peg out a claim for a new hypothesis of Bignami's own, which, if proved by someone else, would give him, and not Manson, the credit. This indeed is an old device of Italian literature of this class. An alleged flaw is

found in a given theory or investigation; the whole theory or investigation is thus discredited; and the critic then proceeds to repeat it in almost the same terms, as now being his own—perhaps with a few alleged experiments in support of it. I was not impressed by the paper because, with Manson, I was already absolutely certain that the flagellate body is not caused by dying struggles but is a living object. But the point was fundamental in our work.

I must have received Bignami's article early in November, and before the end of the month I was able to inform Manson that I had completely disproved his dying-struggle hypothesis. I placed a small lump of vaseline on the patient's finger and pricked his skin through it, so as to allow a drop of blood to exude into the vaseline. The whole mass was now removed on to the microscope slide, without exposing the blood to the air for a second, and was then flattened out and examined. I found that the crescents never underwent the change into spheres and flagellate bodies as described in Chapter IX, but that after twenty-four hours they all showed signs of disintegration and death. How was this then? Here the crescents died but they did not show the dying-struggles at all. So much for Bignami. On the other hand, if the vaseline-specimen was opened after only three or four hours, before the crescents had died, and the blood was exposed to the air, the crescent-sphere-flagella change went on just as before in blood immediately exposed to the air or contained in the mosquito's stomach. That is, the process was certainly a living process. But at the same time I found the great condition necessary for this metamorphosis—which was abstraction of water from the blood, either by exposure to the air or by the stomach of the mosquito. Blood exposed to the air throws out water vapour with great rapidity, and it is certain that the mosquito's stomach absorbs water from the contained blood with equal rapidity. If no abstraction of water takes place the crescents do not become transformed. That is, the crescents become transformed only when the conditions which obtain in the mosquito's stomach are present. On 14 November I had already shown that the same transformation goes on in the stomach of leeches—in which of course a similar abstraction of blood occurs; and I asked Manson to confirm these observations and said, "By the way, could you not work the leech in London?" Indeed, all his work could have been done in London, and Manson and Manson and Rees confirmed it later.

By 7 December I had repeated these experiments over and

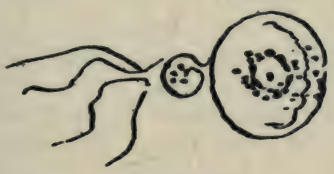
over again and finished my paper, Observations on a Condition Necessary to the Transformation of Crescents [25]. I had also shown that the longer blood is exposed to the air the more of the crescents become transformed. I received much assistance at that time from Dr. Srinivasa Rao. We showed that the flagella generally appear about eleven minutes after the blood is drawn, and ascertained many other interesting details, such as the exact manner in which the crescent turns itself into a sphere and then bursts the envelope of the corpuscle containing it. I was rather indignant with Bignami, and wrote in the following angry and exaggerated way about him: "He wants to secrete a mosquito theory of his own. He is wild with you. He wants to bite into the heart of your theory, suck its juices and then bloat and swell into a discoverer—or rather until he is thought to be one . . . he is welcome to his epidemiological inductions; they will only lead him face to wall against somebody else's. But he must be dealt with at once with clear distinctions because he is quite capable of spreading his six legs over your work and calling it all his own [1]; next he will say that he knows about the flagellated spores all along and so on. If you have not squelched him already you ought to do it." Unfortunately, just after I had posted my article to *The British Medical Journal* I found one by Dr. Marshall in *The Lancet* of 24 October 1896, in which he showed that flagellation is expedited by *adding* water to the blood. This was really due to a mechanical effect of the water, but my paper was delayed by the necessity of investigating the point thoroughly. I have no space to quote the numerous letters to Manson on these points, dated 15 December, 23 December, 27 December (with which we added a note to my article), 11 January 1897, 8 February, 7 March, and 17 March —my last one from Bangalore. My article with appended note appeared in *The British Medical Journal* of 30 January 1897 with a laudatory editorial paragraph; and other notes on the same observations were given before the South Indian Branch of the British Medical Association on 29 January 1897 and were published in *The Indian Medical Gazette* a year later. All these researches will interest chiefly biological workers only who will find full details in the papers mentioned [25].

After I took charge of the Bowring Civil Hospital in June 1896 I began to study microscopical cases of diarrhœa and dysentery, and immediately found numbers of amœbæ and flagellates. Of course work on these organisms had to be done

[1] Exactly what he tried to do [38] (page 339).

during moments snatched from my sanitary labours and my malaria researches, and led to nothing remarkable. But I was one of the first in India to deal with these parasites. D. D. Cunningham in Calcutta had long worked on them, but had gone quite wrong and thought that they were innocuous, thus holding back the hand of science for years. I have no space to deal with the matter here, but may mention my paper (full of misprints) published in 1897 [26].

During all these researches I had never found that more than 60% of the crescent spheres emitted flagella even under the most favourable conditions, the remaining 40% or more continuing to exist as unchanged crescent-spheres for hours. I could think of no explanation of this, though there was a very simple one—the former were male cells and the latter female cells. The " adventures of a flagellum " which I had seen (page 166) ought to have opened my eyes; and on 16



February 1897, shortly before leaving Bangalore, I observed something which should certainly have revealed the truth to me in a flash. In the blood of a case of quartan (*Plasmodium malariæ*) on that date two parasites were lying close together, one emitting a number of struggling " flagellæ," and the other remaining a perfect sphere with one of the flagellæ *moving slowly within it.* Here is a copy of the rough drawing on page 87 of my notebook. Of course only one sperm enters the female cell; but it did not occur to me that this one had *entered* the sphere—I thought it was *trying to get out!* I was so obsessed by Manson's hypothesis that these sperms were flagellated spores that at that time I never even fancied they could be anything else, though previously I had guessed they might be sperms. The honour of this discovery therefore fell deservedly to W. G. MacCallum in America, who actually witnessed some few months later a " flagellum " entering a " crescent-sphere " [28]. I have always felt disgraced as a man of science ever since! Simond had previously proved a similar phenomenon in *Coccidium oviforme* of rabbits [27] (pages 251, 264).

Regarding birds' malaria I wrote to Manson on 31 Octobe 1906: "I forgot to tell you that I have tried several pigeons doves and parroquets with negative results. This place i 2,700 feet above sea-level, remember. However I will loo again. I have not been working with animals. I am dyin to get away to some regular hotbed of malaria. . . ."

CHAPTER XII

THE DAPPLED-WINGED MOSQUITO. SIGUR GHAT, 1897

In organising the new Health Department for the Civil and Military Station of Bangalore I had arranged for a Health Officer; but the salary which could be afforded was not sufficient to provide for an officer of one of the government medical services, such as myself, even if one could be spared. When Surgeon-Colonel Dobson took his furlough home in May 1896 he had applied to be promoted from the Residency Surgency at Bangalore to a better paid one in Rajputana, and had hoped that I should be given the post which he was to relinquish, so that I might continue to superintend Bangalore sanitation for years. Of course this would have been a good thing for me, but it was not to be, because on his return from furlough Dobson found that the Rajputana billet did not suit him, and therefore decided to revert to his former appointment—as he was entitled to do. In March 1897 therefore I found myself left with no better employment than my own appointment as medical officer of my regiment the 19th M.I., which was still at Begumpett, Secunderabad. But before rejoining it I took some short leave which was due to me, and left Bangalore on 29 March 1897 to join my wife and family at Ootacamund—where they had been for a month in a pretty little house which we had taken there, called Blenheim.

At that time the government medical services in India were distributed as follows. The British troops were in the charge of the Army Medical Service, of which there were then, I think, about 400 officers in India—who worked chiefly at the big station hospitals, one of which was in each of the garrison towns or stations. The Indian Medical Service (to which I belonged) consisted of about 600 officers and was divided into two branches. The military branch had charge of the Indian troops, each regiment of which possessed its own medical officer; and the civil branch provided the civil medical and sanitary officers throughout the country. We were

primarily a military service, the Civil Surgeons being always subject to recall by the military in case of emergency. The Civil Surgeons saw most of the medical and surgical practice, especially in the great cities; taught at the medical schools; and had made the service famous by their fine work, especially as regards surgery of the eye, of elephantiasis, and of stone of the bladder. They were generally busy men; but the regimental surgeons, of whom there were often three or four or more in the same station, were seldom occupied for more than an hour or two out of every day—as I have described in the preceding pages. The authorities were obliged to keep up what appeared to be an excessive staff of doctors, partly because of large and sudden demands for frequent military expeditions, and partly because of our rules for leave—we were (quite necessarily) allowed one year's furlough to England after five years' service in India, as well as two months' "privilege leave" every year, and ten days' or three days' "station leave" when we could occasionally be spared (besides sick leave, of course). All this meant that each regimental medical officer was sometimes in charge of several regiments at once, and also that we were often posted about from one station to another at short notice and at great expense to ourselves—since Government frequently refused to pay the full cost, on one excuse or another. As regards general administration, each Presidency possessed its Civil Surgeon-General, a senior I.M.S. officer, with his Secretary, a more junior one; and also a military Principal Medical Officer with his Secretary, both of whom might belong either to the Army or to the Indian Medical Service; while there were also Administrative Medical officers of numerous Districts and Commands. Then there were large numbers of Assistant Surgeons, Hospital Assistants, Compounders, and so on—altogether a very large organisation which, though defective at certain points, did a vast amount of good work. The principal defects were that there was no proper, or indeed scarcely any, provision or encouragement for scientific investigation; no really adequate sanitary service; and no sufficient means for dealing with epidemics. Thus, though plague had broken out for some years in China, almost no precautions had been taken to exclude it from India, so that when it entered in 1896 it fell like an avalanche over the whole country. And in 1897 another demand was made on the medical services by the Tirah (Afridi) campaign in Northern India. These circumstances reduced my chances of being placed on special duty for the investigation of malaria.

In Chapter IX I mentioned how my employment for such investigation by the Maharaja of Patiala had been refused by Government in 1895; but in Bangalore in 1896 I strove hard to be put on special duty for the purpose, after my sanitary work there was concluded. Manson in England pushed hard in the same direction and wrote to many influential people about it—especially to Surgeon-General Cleghorn, Director General of the Indian Medical Service. In August 1896 the United Planters' Association held a meeting at Bangalore, and on the 14th, after considering papers submitted by me, resolved, on the motion of Mr. J. G. Hamilton: "That the Secretary be instructed to move the Government of Madras to commence an enquiry into the development of the malarial parasite on somewhat similar lines to the investigation undertaken by Surgeon-Major Ross and others. . . ." In October, the Hon. Mr. Bliss, Member of the Madras Executive Council, under whom the Civil Medical Service was placed, came to Bangalore. I called upon him at the Residency and he promised to help. The result was that on 30 October the Surgeon-General at Madras (Civil, Dr. Sibthorpe) wrote a handsome letter to the Secretary to the Indian Government at Simla asking for me to be specially appointed for the work. While this was being considered at Simla that able officer, Surgeon-Colonel W. G. King, Sanitary Commissioner at Madras, wrote several despatches urging the same enquiry and pointing out that a place called Pudupett would be suitable for it.

I had now long begun to think that the "brown" and the "brindled" mosquitoes with which only I had worked hitherto (that is, gnats of the genera *Culex* and *Stegomyia*, see page 219) could not be the proper malaria-bearing species; and on 31 October 1896 wrote to Manson: "I have lately been thinking that the first thing necessary to further investigation is to be sent to a place where I can be pretty sure that the species of mosquito is a malaria-bearing species. I was much struck on reading the Goulstonian lectures by the supposition that each species of parasite may require a different species of mosquito [this proved to be wrong] and it is indeed otherwise likely that only certain species can harbour the malaria [this proved to be right]. Here for instance, though mosquitoes are plentiful, there is but little malaria. This may obviously be caused by the species of insect, the brown and the brindled, being not the right kind; and if such be the case I cannot expect the flagellated spores to live in their tissues, so that I cannot hope to find malaria parasites in their tissues nor to produce malaria

by means of them. . . . What is to be done then ? My peons don't seem to be able to get any but the brown or brindled mosquitoes, and as these, especially the latter, swarm in every house, while yet there is little malarial fever, I don't suppose that they are malaria-bearing species. The only thing to be done is to go to some very malarial locality, say a malarious village at the foot of hills. . . . This, accordingly, is what I have asked Government to do with me." And I told him that Mr. Bliss was distinctly favourable ; detailed my plans for the investigation ; and again asked for literature on mosquitoes (none was to be obtained !). I added that I was examining the blood of pigeons, doves, and parroquets for the avian *Plasmodia*—none could be found.

Now, even after five months, no sanction of the investigation had reached me from the Government of India. So, consigning Simla to the plains, I determined to carry it out at my own expense, put my things together, hired extra servants, and asked Dr. C. McVittie (the military Surgeon-General, who was then at Ootacamund) for two months' privilege leave from my regiment at Secunderabad (I had had none since 1893). As everyone knows, Ootacamund is a bit of England placed on the rounded tops of the Nilghiri Hills, about 8,000 feet above sea-level—open grassy downs with woods of eucalyptus here and there, and a lovely climate, without malaria ; but the disease abounded in the gullies and trenches which led down from the summits to the surrounding plains. Just when I was ready, however, the Principal Medical Officer at Secunderabad refused my leave owing to scarcity of officers (one of them was then at Ooty *racing*) ; but the Surgeon-General overruled him. I wrote :

BLENHEIM, OOTACAMUND,
13 *April* 1897.

DEAR DR. MANSON,

Here I am within a week, I hope, of going into the jungles after the malaria parasite. Dobson relieved me a fortnight ago at Bangalore and I intend now to devote my future to real work. The Government investigation has not yet been sanctioned, but I believe it will be sanctioned. I enclose a little fillip I have given them in the *Pioneer* (9 April).[1] But I have no intention of waiting for them to make up their

[1] Merely a short leading article on the necessity for an investigation.

very sluggish minds, and have applied for two months' leave, so as to be able to start work at once. The Surgeon-General tells me that he will give the leave. My apparatus is ready packed for the jungle work, and as soon as I get my leave definitely I shall commence work at the foot of these hills at a place where I hear fever is very bad. This place is at the bottom of the Sigur Ghat (Cañon) the top of which is only half an hour's run from my house here (on a bicycle). I think however that I shall go and live at a dak bungalow lower down the ghat and just above the malaria. However I intend to make no definite plans until I have looked about a bit. Some weeks will be occupied, I expect, in such "looking about"; and I must probably buy tents and get right into the haunts of King Malaria. Tippoogadoo is the name of the place I have heard of. Will write more next mail, if things go well. Am dying to be off, but can't start till definite orders come.

With kindest regards,

Yours sincerely,

R. Ross.

The facts about the sanction for the investigation from Simla were these. On arrival at Ooty I wrote to the Secretary to the Civil Surgeon-General at Madras (Dr. Sibthorpe) asking if the sanction had been received. On 22 April he answered: "I am desired to say that no reply has yet been received from the Govt. of India on the subject of the malaria investigation." To this letter I appended the note: "I read the reply myself at Govt. offices Ooty on the 21st!" Indeed the reply had been sent from Simla on 19 March, but had not been communicated to me all this time! This delay was a misfortune for me, because on 15 April the secretary of Dr. Cleghorn, Director-General of the Indian Medical Service at Simla, had wired to me: "Will you be candidate for officiating civil surgeoncy at Akola wire reply." I refused, because of the expected malaria investigation—so that I lost the appointment owing to this piece of official incompetence. I found the papers at Ootacamund on the 21st on personal enquiry at the office, and still possess all the documents. Doctors are often shocking bunglers at official correspondence. Of course, when offering me the appointment, Dr. Cleghorn's secretary should have wired to me that the malaria investigation had not been sanc-

tioned. But misfortunes do not come singly, as the following will show.

OOTACAMUND, 28 *April* 1897.

DEAR DR. MANSON,

My reconnaissance has had rather an unexpected ending. I went to Kalhutti on Thursday 22nd. On the 23rd I went down to the bottom of the ghat (4 miles) and then 4 miles across the spur of a mountain to another valley where there is Kindersley's coffee estate, Westbury. We had had early tea at 7.30 a.m., started from Kalhutti at 8 a.m. and reached the coffee estate at 10.30 or 11.0. Then we had tea which I noticed was coolish, while the milk seemed watered (?). Then we examined a boy with fever (finding malignant tertian) and went to look at the source of drinking water, a puddle fed by a mountain stream or rather a trickle. The puddle was full of frogs and water beetles, etc. Next we walked on the estate which was under irrigation, the ground and air teaming with life and seething with heat and moisture. We had breakfast at 1.30 (whiskey and soda). More coolies examined afterwards; many with spleens; range at 6 p.m. On Saturday I examined specimens and on Sunday 25th did the same with several walks in the upper part of the ghat. At 10 p.m. I was seized with a severe go of fever and had to come back here yesterday.

The fever was bad all night beginning with a sudden pain in the liver followed by 2 hours' rigor and then two hours' fever with aching in bones and gradual defervescence. I slept from 4 to 6 a.m. on the 26th and woke feeling all right. I had taken 5 grs. quinine on the 22nd, 23rd and 24th, but 10 grs. on Sunday the 25th. As soon as I knew what was wrong I took 20 grs. more at once. Hence on Monday morning I found only one small amœboid body (quite typical and moving) in two excellent specimens. I don't know the species. At 9 a.m. fever began to come on again without rigor and very slightly. I slept till 3 p.m. when I woke in much perspiration and have not had fever since. Liver still hurts on a deep breath but I gave the parasites such a warm reception with quinine that I don't think they liked it. I shall be off down to the ghat again in a few days. Unfortunately had no thermometer with me at Kalhutti, but the fever must have run very high because I felt

twitchings all over and was a trifle light-headed I think for a little. Had no phenacetin, etc.

Now I slept every night at Kalhutti inside a mosquito net and with closed windows and doors. At Kalhutti there is a planter, Nash, with a few coolies two of whom had been ill lately with fever. In the dak bungalow where I was the native servant and his wife had both fever lately. He had a large spleen but no parasites. In front of their hut was a puddle full of mosquito grubs which I am still examining. Neither of my two servants who also slept at Kalhutti have had fever yet. We all drank boiled water and milk religiously.

I must watch developments before judging where I got the fever but think I got it at Westbury, Kindersley's estate at the foot of the ghat on Friday the 23rd. This gives about 60 hours incubation period. Anyway the incubation must have been under 72 hours. Kalhutti is 5,500 feet up above the sea and Westbury about 3,000 feet. 29th. I fear this letter has missed the mail. My case gives useful data to go upon. Assuming the incubation period to have been 48 hours, then with the most rapidly developing species of parasite, no more than two generations could have hatched in that time; in other words on the average one parasite admitted on the 23rd could scarcely have become more than 100 parasites on the 25th. Again, on the 25th I suppose that quite one in 10,000 of my corpuscles must have been infected, probably many more. Now I have about 25,000,000,000,000 corpuscles [1]; so that on the 25th I must have had about 2,500,000,000 parasites in my body. Dividing this by 100 we get the number of parasites which I must have taken in on the 23rd, namely about 25,000,000.

I think you will admit on consideration that this line of reasoning is really quite sound fantastic as it may seem.[2] We can roughly estimate the rate at which a quotidian parasite can breed from the number of spores it has and the period of the cycle. All this considered, I think that 25 millions of parasites or spores must have been the least number admitted on the 23rd.

[1] A wrong estimate based on old data. Now physiologists would say that I have about 15,000,000,000,000 red corpuscles.

[2] Manson, who was not mathematical, feared I was still "light-headed" when I wrote all this!

Now on that day I spent nine hours in the malarious region and breathed about 30 inhalations a minute, each inhalation containing about 25 cubic inches of air (vide any physiology textbook). Hence I breathed about 405,000 cubic inches of air during the nine hours. Suppose all the air to have been equally impregnated with malaria germs the 405,000 cubic inches must have contained 25,000,000 germs; or one cubic inch must have contained no less than 62 malaria germs.

This is something unheard of, especially as I was in the malarious region only during the day. The largest number of bacteria found in air is I believe something like one bacterium in 10 cubic inches. Here we have 62 sporozoa in one inch! If however I contracted the infection while walking in the coffee plantation (one hour) the number of germs there must have been nine times as large namely about 558 a cubic inch!

On the other hand if I contracted this infection by drinking the tea, of which I consumed about 1½ pints, we should still get a large number of germs per cubic centimetre but nothing unheard of. Anyway the air or water of the locality must contain large quantities of the material which causes malarial infection. I may add that my case of attack after 48 hours agrees perfectly with report and tradition which speak of numerous persons who have died straight off a day or two after having passed through the lower ghat.

All this appears to me to have a most important bearing on the great problem because it makes us expect to find large numbers of the free form of the germ, whatever it is, in the air and water, and ought to facilitate the search. I examined the Westbury water carefully. It did not contain a single mosquito grub nor did I find one single psorosperm in the mud. . . .

Yours sincerely,

R. Ross.

The line of mathematical reasoning, here adopted for the first time, was perfectly correct. It was much amplified later in my *Prevention of Malaria* [91], and has led recently to my method of curing parasitic infections by " continuous medication " [98]. Certainly I had an attack of malarial fever on Sunday, 25 April 1897, but how or where I became infected I do not know for certain. Even if I was infected on the night

of 22–23 April, the incubation period would have been only 72 hours—an extraordinarily short time—it is generally two or three weeks. But there is a case on record of two or three days' incubation period after the experimental injection of malarious blood very rich in parasites by Bignami (see case 25 in my *Prevention of Malaria*); and if my case was a similar one I should have received an enormous dose of the poison somehow or other on the 22nd–23rd. Of course at that time I had only begun to suspect that infection was carried by the *bites* of mosquitoes (page 190), and still supposed that it lay in drinking water, according to Manson's ideas; so that I thought that I must have drunk some very heavily infected water at Kalhutti. Now we know that infection is carried only by the bites of mosquitoes. Hence if I was infected on the 22nd–23rd I should have been very frequently bitten then. But, so far as I knew I was not bitten at all then, and very little later at the foot of the ghat. Was I infected previously in Ootacamund? Just possible, but not likely. On jogging my memory, however, I now recollect that I went to look at the Sigur Ghat, to see what it was like, some time before I first went to Kalhutti on the 22nd—quite possibly about the 14 April. I remember that it was in the evening and that I bicycled some way down the road to Kalhutti; but I cannot remember whether I reached Kalhutti itself on that occasion or not. Certainly there is no note of my having done so, either in my notebook (in which other details are carefully recorded) or in my letters to Manson. On the whole I now think it most probable that I was infected by the bite of an infected *Anopheles* on that evening, either at Kalhutti or on the road there.

On 27 April I returned home from Kalhutti, a wreck; but I had little more fever, though I found a malaria parasite again in my blood on 29 April. After that I took 10 grains of quinine every day for no less than four months and never had a relapse; but I remained weak and depressed for a long time. In spite of this, however, I worked hard at my specimens and visited the Sigur Ghat again on 5 May and 1 June. On 3 May I wrote Manson a long letter, in which I argued that, as I had found so few mosquitoes at Westbury and yet had contracted malaria so quickly, the malaria parasites in their free stage must be able to multiply enormously in external nature, and must be capable only of optional (i.e., facultative) parasitism in men and mosquitoes. But this was too hasty a conclusion, for in my next letter I wrote:

Ootacamund, 12 *May* 1897.

Dear Dr. Manson,

Another five days in the jungle have reversed the position of affairs in favour of the mosquito theory, but only just as I was beginning to give it up. I started to Kalhutti again on the 5th. On the 6th I went to Sigur, the bottom of the Ghat, and spent the whole day there hunting for mosquito pools and examining dew, puddles, etc. Not a mosquito grub to be found. There is a rest-house there and in it I found a native with fever. He said he had arrived on 17 April and had been attacked with fever four days' afterwards (he may have contracted it on the way). On my intimating that I wanted to examine his blood after breakfast he disappeared. I found however a family of nine Kusbahs living all their lives at Sigur in a spot supposed to be absolutely deadly: not one of them had ever had fever, they said, and certainly none of the seven men and children had spleen. They drank river water. I encamped at this spot during the day on the 6th and 7th inst. returning four miles up the ghat to Kalhutti in the evening. I found not a single mosquito grub anywhere but caught a mosquito full of blood in the rest-house. This mosquito was small with wings striped *brown* and white.[1] The Kusbahs (aborigines) informed me that sometimes mosquitoes are so numerous at night that fires are required to keep them off.

On the 8th I descended again and went to Nash's estate two miles out in the plain beyond Sigur. Here thirteen out of sixteen people had large spleens or fever. They drank out of irrigation streams and lived in the midst of jungle. Even here I could not find a single mosquito pool. The same evening I went on to Westbury, three miles from Nash's plain estate, with intent to live there for a few days (this is where I expect I got my fever at first).

On Sunday 9th I examined water of puddles, mud, everything—not a sign of a mosquito. There was not a mosquito in this bungalow either. I then offered a reward for every mosquito alive or dead, brought to me and went to bed feeling that mosquitoes could play no essential part in the propagation of malaria.

[1] The wings were spotted, not striped. It was the first *Anopheles* I had ever observed !

On the 10th (Monday) I returned to Nash's estate and examined bloods—one case of malignant tertian ; looked about a little more and went back to Westbury. Here about 80% of the people have spleen. Out of 19 Kusbahs I found about half with spleens or fever (women would not be examined). Hence the Kusbahs are by no means immune, though they are partially so, compared with other natives on the estates who had come from distant parts. Hence again the fact that the nine Kusbahs at Sigur are free from fever appears to show that that part is free from fever though Nash's estate and Westbury close by, are so bad. The river at Sigur, from which the nine Kusbahs drink, is however a mountain stream running over stones and sand ; the streams at Nash's and at Westbury run through rotting vegetation.

After breakfast on the 10th an intelligent native brought me five very small mosquitoes. I jumped with astonishment at the sight of them and told him to take me where he had found them. Instead of taking me to the servants' huts he led me into the neighbouring jungle. It was then midday. He sat down ; in one minute four or five mosquitoes had fastened on his black legs and arms, one was on my hand and several were prospecting my trowsers. I let the one on my hand bite me, which it did in a few seconds, giving a sharp pain like a bee sting. One on the native's legs filled itself in about a minute. These insects abounded in the darkest parts of the jungle, which however was not a large jungle but rather thin scrub and thorn. I searched everywhere for the grubs but could not find a pool of water anywhere, though it had been raining over night. Anyway the adult insects swarmed everywhere in the wood. In the evening we looked again for grubs, and at last discovered some in a small pool in a rocky ravine almost entirely sheltered from the sun by thick overhead vegetation and fully half a mile from where we had first found the mosquitoes.

Hence enter Culex Silvestris or Nemoris ! [1]—which I ought to have thought of before. Oddly enough there were none under the tree at Sigur where I encamped. The fact is they

[1] Probably a very small variety of *Stegomyia scutellaris* with the same parasites, eggs, and general appearance. I expect that it was breeding in "rot-holes" in trees, and that the larvæ found in the ravine were larvæ of some other species. I have forgotten details.

do not frequent single trees but shady glades. They appear to fix on one with voracity morning, noon, evening, or night. They do not enter houses—at least well-made ones—though the natives say they come into their huts at night. They breed apparently only in the darkest and most secluded woodland pools in dried-up-water courses and are quite different from the ordinary house-mosquito [? in size only].

The adults are very small with brindled bellies and very formidable suctorial apparatus. The grubs are also small, brindled and with very long air-tubes. The adults contain swarms of psorosperms, but in the few which I have yet examined I have not found anything very unusual. . . . Well then, the Culex Sylvestris has entirely altered the state of affairs. Where I was just about to give up the theory owing to the absence of mosquitoes I find that mosquitoes are swarming. *If then Malaria occupies the mosquito at all it occupies the Culex Sylvestris* and I must find it there by hook or by crook. . . . It looks also as if the bite does it in some way, it appearing to me very questionable whether the forces of dilution in air or water would not reduce the chance of infection almost to an impossibility even granting that the woods are full of the Sylvestris. It appears much more likely that infection is brought straight to men (as a rule) by the insect itself. . . .

Yours truly,

R. Ross.

Here is an example of the numerous pitfalls which beset the path of the investigator. This swarming *Culex sylvestris* does not carry malaria at all; but the single insect, " with wings striped brown and white," which I found in the rest-house at Sigur was the real culprit! I remember the latter well. Unlike most of the genera *Culex* and *Stegomyia*, its wings were *spotted;* and I noticed at once that it sat with its tail sloped at an angle to the wall, while the abdomens of *Culex* and *Stegomyia* hang parallel to the wall. It was in fact an *Anopheles*, the malaria-bearing family of mosquito! But this was actually the first time that I had ever seen (or at least noted) one, though I had now been studying mosquitoes and malaria for more than two years. Who would have thought that this " single spy " was the true enemy, while the whole " battalions " of the *Culex sylvestris* were innocent? Another four months were however fated to pass before I could convict

it. On the other hand, my conjecture that " the bite does it " was right; but another year was to pass before I could prove it so. The mosquitoes which came into the huts of the natives at night were probably the *Anopheles* and not the *Culex*.

Four more long letters to Manson from Ootacamund dated 16, 24, 31 May, and 7 June 1897 were all occupied with minute descriptions of a number of new kinds of parasites which I found in the Sigur mosquitoes—any one of which might have been, for all I knew, the mosquito-stage of the malaria parasite. I will not detain the reader with descriptions of these, though they were interesting enough, because some account of them was published in papers to be mentioned presently. In the last of these letters I now gave a list of six kinds of parasites of mosquitoes, all of which except the first were discovered by me, namely,

1. The *Filaria bancrofti* (discovered in the mosquito by Manson).
2. A nematode worm.
3. *Gregarina culicis* (probably several species).
4. *Coccidium* (*?*) *culicis*.
5. *Octosporidium culicis*.
6. Certain flagellate organisms in the stomach and intestine.

I saw also three other classes of objects which might have been parasites. On 14 May I gave numbers of the flagellate parasites to one of my servants by the mouth. He had a slight attack of fever on the 20th; but no malaria was found in him, and I heard subsequently that he was subject to such attacks. Of course he received the usual reward for a " positive result."

My work in the Sigur Ghat proved to my mathematical apprehension that I was " up against " a very difficult problem indeed—an equation containing *two* unknown quantities. There were many species of mosquitoes, and each of these contained many kinds of parasites. How was I to discover which species of mosquito and which kind of parasite were the right ones? To find one unknown quantity I should be obliged to find the other one simultaneously. There was only one method of solution, that of incessant trial and exclusion. But this meant enormous labour—and I had already spent two years over the quest.

I wrote an account of the work in a paper [31] which I sent to the South Indian Branch of the British Medical Association some time before 4 September 1897—when the secretary informed me that it " was considered too long to be read in its entirety

before the Branch." It contains thirteen small pages ! Publication was delayed until February 1898, but then it was reproduced in *The Indian Medical Gazette* for May and June 1898 (many printers' errors). I made a mistake in sending so many of my papers to a society which dealt with little else than clinical wonders—such as what I called " wah-wah tumours " —and the proceedings of which were not widely read. This paper was important because of my analysis of the facts hitherto obtained. I inclined to the view that *contact* between men and mosquitoes is the mode of infection ; and by contact I meant that the mosquito either inoculates the germ into the wound made by its proboscis or deposits it when defæcating on the skin of its victim. On the other hand I admitted that, at least, I had found no further evidence for the mosquito theory in these investigations. Many of the parasites which I saw have since been rediscovered and named by several observers ; but I gave a résumé of the subject in *The Journal of Hygiene*, Cambridge [82], in 1906.

I was now forty years old and had had sixteen years' service ; but though I was well known in India, both for my sanitary work at Bangalore and for my researches on malaria, I received no advancement at all for my pains. By a fixed rule an officer passing from one branch of the service to another was obliged to begin at the bottom of the list in the latter branch, so that I was practically excluded from entering the civil side of the Madras medical service. Owing to the kind interest of Colonel Robertson, the Resident at Bangalore, I was offered an appointment in Rajputana ; but this and the appointment at Akola in Berar were by the same rule only very inferior temporary posts ; and I saw myself condemned for the rest of my service to nothing but regimental work at the lowest salary (for my rank) of about 800 rupees a month. What angered me still more was that not one of my colleagues seemed to take the smallest interest in the malaria work—except Surgeon-Colonel King. Even the heads of the service in Madras, Surgeon-Generals Sibthorpe and McVittie, did not deign to come and see my specimens at Ootacamund, though I begged them to do so (how little they were interested will be gathered from what follows presently). On the other hand, many of the best appointments were filled by men who had really done no special work at all and never intended to do any. When I asked for an extension of leave to continue the malaria work I was refused, though a colleague had been granted six months' leave there for racing. In short, my prospects were not good

enough, and I determined to retire on my first pension, which would be due to me in a year. But first I wished to make one more desperate effort to solve the Great Problem, and therefore wrote on 12 and 20 June to Manson describing my position and begging him to try to have me placed on one year's special duty for this almost forlorn hope, before I left the service. I was much discouraged and far from well.

So far as I remember I wrote no verses during my " holidays," except the sections of *In Exile*, labelled " Truth-service " and " Self-service," and " Wraths "—bitter, perhaps, but justified. Somewhere else, however, and I think previously, those words which come near the end of the poem had been composed :

> By that we have we lose ;
> By what we have not, get ;
> And where we cannot choose
> The crown of life is set.

So it was indeed. I could not choose but to return to Secunderabad ; and there, two months later, the Great Problem was to be solved !

CHAPTER XIII

THE DISCOVERY. SECUNDERABAD, 1897

LEAVING my wife and family at Ootacamund (where the latter all caught whooping-cough a little later), I started on the 16th, spent the 17th at Bangalore, and reached Secunderabad at 6 p.m. on 18 June 1897.[1] There I rejoined my regiment the 19th Madras Infantry at Begumpett Lines, situated near the outflow of the Husein Saugar Lake (page 101), and lived in a good bungalow with Lieut. Heffernan, the adjutant, and another officer. My pay was the usual 800 rupees a month, but was reduced by quite 100 rupees a month by regimental expenses, as I was the next senior officer to the commandant; so that I drew only about half the salary I had in Bangalore. The weather was pleasant when I arrived, but soon, instead of the cooling monsoon rains which ought to have fallen by that date, there came only a raging hot wind with clouds of dust which shrivelled us up. The life itself was pleasant, and all the officers, Major Packenham, Captain Harding, Lieut. Knox, and others, and the ladies of some of them, were very nice people. In the evenings we played cricket in the maidan opposite the mess and then went to the club; and instead of a pony and trap I used my excellent bicycle. All my doings during this eventful time are recorded in almost daily letters to my wife, beside numerous letters to Manson.

My first experience was far from pleasant. There was a guest-night dinner at mess on 25 June, and we enjoyed a delicious ice-cream pudding. The agony commenced a little after midnight. All five of us suffered—and so, we were glad to hear, did some of the servants (whose caste prejudices forbade them to touch our food !). Cholera was in the station, and Major Fawcett, the Staff Surgeon, said next day that he considered we had got that disease—but as bacteriology had scarcely reached India yet (seventeen years after the discovery of the cause of cholera) we did not know for certain. The same

[1] Wrongly put July 1897 in my Nobel Lecture—which was written chiefly from memory and might have been more exact in some small details.

cream had been provided for another dinner party on that night, and the wife of our brave general, General Tucker, afterwards of South African fame, was attacked at the same time and died a week later. Mine was the most serious case of our batch, and I kept myself alive by drinking cup after cup of scalding hot tea without sugar or milk, swallowing another cupful as soon as a previous one had been rejected. I had thought out and tried to use this line of treatment during the cholera outbreaks in Bangalore in 1896—the object of it being to maintain the water-content and the pressure of the blood, on the lines of the intravenous injection of salt-solution so successfully employed later by Sir Leonard Rogers. Next day Dr. Fawcett looked very grave, and I felt intensely cold for some days afterwards, as I did in my previous similar attack (page 54) in Madras.

My next adventure was more amusing. As soon as he heard that I had arrived in Secunderabad, Surgeon-Colonel Lawrie (page 178) challenged me, not to a duel, but to a mortal logomachy over the malaria parasite. I was to bring my own cases to Chudderghat (some eleven miles from Secunderabad) and to demonstrate the *Plasmodium* before him and the full Faculties and students of the Nizam's Medical School in formal debate assembled. Well, I found a good case in my hospital, and on 11 July at 8 a.m. the man's blood was swarming with the youngest stages of *Plasmodium falciparum*. I put him into a bullock-cart with a hospital assistant and a handsome tip, despatched him to Chudderghat, wired to Lawrie, and followed after breakfast. We arrived about noon. Lawrie came into the arena (a lecture room) with about thirty native professors and students, all looking very serious. Not a single parasite could I now find in the patient's blood ! Of course, as the Italians had shown, the young stages of this species retire into the inner organs some few hours after their first appearance, but, though I was familiar with this phenomenon, I did not expect the disappearance to be so speedy. It was not only speedy but complete ! Lawrie was very polite ; but some of the students tittered. I was absolutely routed and did not even dare to describe my defeat to Manson. Not as with prophets, do the miracles of the scientist always succeed. It was like St. Paul before Agrippa, but I did not almost convert Lawrie to " Laveranity." He was indeed one of the many good surgeons and passable doctors who have no clear conception of what science means.

There was next to no official work to do in the regiment,

and therefore a junior Indian officer of the I.M.S., named Dr. Kanga, was appointed to help me to do it. There were numerous doctors in the station, and I offered the Principal Medical Officer, Surgeon-Colonel N. B. Major, A.M.S., to instruct as many of his officers as he pleased in the mysteries of the *Plasmodium*. I believe he published a notice to this effect in Station Orders; but, so far as I remember, only two officers came to me at that time, Surgeon-Captain C. A. Johnston, I.M.S., and a Dr. Foulkes, who worked with me for some weeks in July—they and Dr. Sturmer of Madras being, I think, the only government officers who did so. The P.M.O. himself was not interested. Yet malaria was the commonest disease in the country, and was perhaps the principal cause of expense during military expeditions. In fact few men in the whole of India were then doing sound work on the subject. One very good article, "The Malaria Parasites at Lahore," by C. H. Murray, appeared in the *Scientific Memoirs* by Medical Officers in India for 1897; but it did not touch the mosquito-theory, and I did not see it till much later.

On arrival at Secunderabad I indited a report (dated 12 July) on my work in the Sigur Ghat for the Director-General of our service, and on 20 July his secretary, Surgeon-Captain Leslie, wrote to me from Simla that Surgeon-General Cleghorn "considers the paper original and highly interesting, and that, when the state of the service permits, he will endeavour to provide you with more favourable opportunities for carrying on your researches than are at present possessed by you." This gave me great encouragement; and, in fact on 25 June, Dr. Leslie had wired me: "Do you wish to become a candidate for a civil surgeoncy in the Central Provinces?" and then on 27 June had wired again: "You will probably get an appointment under the foreign office soon but cannot say when." I replied that I left the matter in the Director-General's hands, and I felt somewhat reassured but also—well, the reader shall see!

On 27 July I received two letters from Manson in reply to mine from Ootacamund complaining of my prospects. He had taken instant action and sent me a copy of an admirable letter which he had written to Sir Charles Crosthwaite of the India Office in London urging that my malaria work should be encouraged and that I should not be driven to take my pension (which would be due to me on 23 June 1898). I cannot forbear quoting the following fine and exact passage: "To our national shame be it said that few, very few of the wonderful advances

in the science of the healing art which have signalised recent years have been made by our countrymen. This is particularly apparent in the matter of tropical diseases in which we should, in virtue of our exceptional opportunities, be *facile princeps*. But even in tropical diseases Frenchmen, Italians, Germans, Americans, and even Japanese are shooting ahead of us. We, have to get a Koch to find for us the cholera germ, and a Haffkine to protect us from it, a Laveran to teach us what malaria is, a Kitasato to show us the germ of plague and a Yersin or a Haffkine to cure us of its effects. . . . But in this matter of malaria here is a chance for an Englishman to rehabilitate our national character and to point out to the rest of the world how to deal with the most important disease in the world—malaria." Manson hoped even to reach those Awful Presences, the Secretary of State and the Viceroy—then Lord George Hamilton and Lord Elgin ; and he succeeded. He added that there was no one so competent in India for the proposed malaria investigation as myself, and that it would take years of toil for anyone else to work up to the point reached by me. As a matter of fact, no one else was working at the mosquito theory at all. Out of the thousand million people in the world I believe I was the only one who had been so foolish as to waste leisure, money, and advancement on such a wild-goose chase. The Italians, French, and Americans were doing nothing at all, and even Manson himself did not try English mosquitoes but left the enquiry to me.

But now the predestined day was approaching, and the Power that " shapes our ends, rough-hew them as we may " took the matter out of the hands of the incompetents. I will try to reconstruct the events as exactly as I can out of my notebooks, letters, and memories. On arrival at Secunderabad after the severe labour in Ootacamund, I felt my first violent reaction against the microscope, and could scarcely bring myself to look through mine for a month. The Great Monsoon seemed to have failed. The hot blast which, instead of it, struck us in June was followed by a suffocating stillness, and the sky was filled with a haze of dust through which the sun glared like a foiled enchanter.

> The desert is white with heat
> Intol'rable, and the smoke of the burning sand
> Fills all the air. . . .
> What cast of thin vapour covers the flaming sky
> And why on his throne so sickly pale the Sun
> Erstwhile of the brazen empyrean lord ?

> What ails him who harms us that his lips are pale—
> Pale, and his javelins wreak his wrath no more ;
> And white his withering lips and his spectral hair
> So scatter'd with fear ?[1]

Well do I remember those awful days—and nights. I spent the time doing almost nothing but (I believe) writing—or rather moulding in the mind—the stanzas of *In Exile*, Part VII [114] :

> What ails the solitude ?
> Is this the Judgement Day ?
> The sky is red as blood ;
> The very rocks decay
>
> And crack and crumble, and
> There is a flame of wind
> Wherewith the burning sand
> Is ever mass'd and thin'd.
>
> . . . . .
>
> The world is white with heat ;
> The world is rent and riven ;
> The world and heavens meet ;
> The lost stars cry in heav'n.

I do not boast my premonitions, because they seldom come true ! but at that time I was certainly much exalted in spirit and said to myself : " One more effort and the thing will be done." I remember especially a dreadful evening, when I climbed one of the heaps of great boulders piled upon each other which dot the plain outside the station, and saw the vulture and the dead jackal (mentioned in the poem) below. Then it was that the thought struck me : Why not see whether mosquitoes fed on malaria blood as before contain any of the mosquito-parasites which I had found in the Sigur Ghat ? I was at full work again on 21 July 1897 on the last lap.

The position of the investigation was now as follows : Manson's theory consisted of two parts. The first part was that the crescent-sphere-flagella metamorphosis was intended to occur in the mosquito's stomach. This was a great and ineluctable induction which I had verified two years previously in this very place. But I had got no further because the second part of his theory—that the liberated " flagella " live in the insect's tissues, from which they enter drinking water—was not nearly so strong, indeed not true. They might, for example, be voided at once by the mosquito on to its victim's skin, or even upon the ground of malarious places. In fact they do none of these things, but enter and fertilise the female *Plasmodia*. Manson, even in his last letters, kept advising me to " follow

[1] From my " Indian Shepherds " [117].

the flagellum," but if I had done so I would merely have returned to the point whence I started. My plan now was simply that of 1895, to examine each " malariated mosquito " for any parasites which it might contain, but to do so much more thoroughly than before, and in the light of my Sigur experiences.

Then the *species* of mosquito had to be considered. In spite of all efforts, I had been able to obtain no useful literature on mosquitoes (*Culicidæ*), and knew no one whom I should address on the subject; and, though I had sent many of the insects to Manson, none of them had been zoologically classified except one, which he wrongly thought was *Culex pipiens*. Hence, pending further information, I had been obliged ever since 1895 to use a rough classification of my own. I recognised two main groups among the more common mosquitoes with which I had been working, namely:

A. *Grey or Barred-back Mosquitoes* (mostly *Culex fatigans*, Wied), which bit at night, had plain wings, and sat on a wall with tail sloped towards it. Their eggs were shaped like rifle cartridges, and were laid packed side by side in clusters, and their larvæ had a long breathing tube and floated head downwards. Very like the common English gnat, called *Culex pipiens*.

B. *Brindled Mosquitoes* (mostly *Stegomyia fasciata* Fabr. and *Stegomyia scutellaris* Walker), which bit in the day-time, had plain wings, and sat like A. But their eggs were oval and laid singly; and their larvæ had a short breathing tube, but also floated head downwards like those of A.

But in the Sigur Ghat I had found (page 208) a single mosquito of quite another type, which I noted, then and in 1898, to have the following characters:

C. *Dappled-winged Mosquitoes* (Sub-family Anophelina), which bite at night, have spotted wings, and sit on a wall with tail sloped away from it. Their eggs are boat-shaped, laid singly, and often lie together touching each other end to end so as to form equilateral triangles. Their larvæ have no breathing tube and float flat on the surface of the water like sticks.

The main zoological character which is taken to distinguish groups A and B (Sub-family Culicina) from group C (the Anophelina) is that in the former the females have short palpi, and in the latter have long ones. The bodies of the Culicines are also much less graceful than those of the Anophelines, which seem to be more suitable for longer flight.

To resume then. About the middle of July I hired three natives to look far and wide for mosquitoes, adults and larvæ, but for a month they brought me nothing but groups A and B. On 21 July a case with fairly numerous crescents came to hospital; and, as I had long thought that these groups were not connected with malaria, I now determined to try them again with the most scrupulous care. On 27 July I wrote to Manson: "The experience gained in the Sigur Ghat has given me confidence and knowledge. . . . I am of course sticking to the flagellulæ again but on a slightly modified principle. Owing to their extreme delicacy I doubt whether they can be directly followed into the insect's tissues; but this difficulty can be avoided in a simple manner. The question I have set myself to answer, and which I now feel myself able to tackle, is 'Do healthy mosquitoes, fed on malarial blood, contain any parasite which similar mosquitoes fed on normal blood do not contain?' . . . I keep mosquitoes, after feeding on crescents, for one, two, three . . . days and then examine, so as to allow the flagellulæ time, by supposition, to mature into full-grown mosquito-parasites. . . . Suspicious bodies being noted, I have only to compare with control insects. . . . Another question however must be held in mind—'Do mosquitoes fed on malarial blood *evacuate* any protozoa not evacuated under similar circumstances by mosquitoes fed on healthy blood?' It may take weeks or years to obtain answers to these questions. . . . I dissect each malariated mosquito with the utmost care and then search every micron of it (a micron is one-thousandth part of a millimetre) with an oil-immersion. Each mosquito takes about two hours' work. . . . The things are there and must be found! It is simply a matter of hard work." On 4 August I wrote: "No parasite has been found. . . . I shall not miss a cytozoon, a spore, an amœba, or a flagellum, believe me," and on 11 August: "No definite results; and I expect that I must soon state: (a) that the brindled species I have been examining are not the hosts of the flagellulæ of crescents or spr. tertian, or (b) the flagellulæ develop in them into something very delicate, or very small, or very rare, or almost indistinguishable from normal cells or nuclei."

The monsoon broke, a month later, in the evening after an awful day, 16 July; and I described the Great Cloud near the end of *In Exile*:

> Art thou an Angel—speak,
> Stupendous Cloud that comest?
> What wrath on whom to wreak?
> Redeemest thou, or doomest?

But the weather became very hot again in August. At first I toiled comfortably, but as failure followed failure, I became exasperated and worked till I could hardly see my way home late in the afternoons. Well do I remember that dark hot little office in the hospital at Begumpett, with the necessary gleam of light coming in from under the eaves of the veranda. I did not allow the punka to be used because it blew about my dissected mosquitoes, which were partly examined without a cover-glass; and the result was that swarms of flies and of "eye-flies"—minute little insects which try to get into one's ears and eyelids—tormented me at their pleasure, while an occasional *Stegomyia* revenged herself on me for the death of her friends. The screws of my microscope were rusted with sweat from my forehead and hands, and its last remaining eye-piece was cracked!

By the 15 August thirty-one mosquitoes of types A and B, all bred from the larva and fed on malarial patients, had been scrupulously examined; not counting numerous unfed mosquitoes, bad dissections, partial dissections, and other studies, including a rat swarming with trypanosomes (thus I had once watched four "flagella" simultaneously attacking a lymphocyte—page 266). On the previous day I had written to my wife: "I have failed in finding parasites in mosquitoes fed on malaria patients, but perhaps am not using the proper kind of mosquito." Now, as if in answer, some Angel of Fate must have met one of my three "mosquito-men" in his leisurely perambulations and must have put into his hand a bottle of mosquito larvæ, some of which I saw at once were of a type different from the usual *Culex* and *Stegomyia* larvæ. Next morning, the 16 August, when I went again to hospital after breakfast, the Hospital Assistant (I regret I have forgotten his name) pointed out a small mosquito seated on the wall with its tail *sticking outwards*. I caught it by my method of placing the mouth of a bottle *slowly* over it—if one jabs the bottle quickly the insect always escapes sideways—and killed it with tobacco smoke. It had spots on the wings,[1] and was evidently like the insect which I had found in the rest-house at Sigur (page 208), and is described in my notebook as "A brown mosquito, not brindled, with three black bars on wings caught in ward." I dissected it at once and found nothing unusual; but while I was doing so—I remember the details well—the worthy Hospital Assistant ran in to say that there were a number of mosquitoes of the same class which had hatched out in the bottle that my

[1] Probably *Anopheles culicifacies*.

men had brought me yesterday. Sure enough there they were: about a dozen big brown fellows, with fine tapered bodies and spotted wings,[1] hungrily trying to escape through the gauze covering of the flask which the Angel of Fate had given to my humble retainer !—dappled-winged mosquitoes, Type C, the first I had ever found in Secunderabad, but larger than the one I had just caught on the wall. Immediately my patient, Husein Khan, a man with fairly numerous crescents (and also *Filariæ*) was stripped and put on the bed under the mosquito-net. This was at 12.25 p.m. by my notebook; and in five minutes ten of the new mosquitoes had gorged themselves on him and were caught by the Hospital Assistant, each in its separate test-tube, with a drop of water to drink and a loose lump of cotton-wool to prevent escape—Husein Khan received one anna for each. At 12.40 and 12.50 p.m. I dissected two of the insects at once in order to watch the crescent-sphere flagella metamorphosis in them. I saw little of it in them, but found a filaria of 200 to 250 microns in length, and with a sheath, in one of them. That evening I wrote to my wife: "I have found another kind of mosquito with which I am now experimenting, and hope for more satisfactory results with it."

Next day, 17 August, two of my new beauties (*A. stephensi*) were dead, but I dissected two of the survivors, mosquitoes 32 and 33. They are described in my notebook as "Large, legs, proboscis and anterior border of wings, spotted dark brown and white—brown spots on tail joint of body. Back of abd. and thorax light brown, belly dark brown. Wings nearly white." I was rather excited over the dissections, spoiled them, and found nothing; and spent the rest of the day examining a flea taken from the rat with trypanosomes.

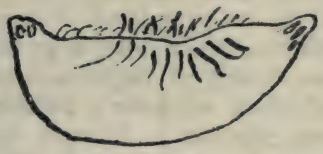
On the 18th no notes were made in my book, but I wrote to Manson: "I am now beginning on a large, voracious light-brown species, with brindled legs and proboscis and thin black bars on the wings—like one of the Sigur species—a kind which I have obtained here with some difficulty." In fact, though I told my mosquito men to remember where exactly they obtained their samples, the fellow who brought me the Anopheles could not recollect the point and could get me no more on 16 and 17 August.

On the 19th there were some more *Anopheles* (*A. stephensi*) recently hatched out in the original bottle,[2] and I fed and dis-

[1] Probably *Anopheles stephensi*.

[2] In my Nobel Lecture these two are confused with the insects fed on the 16th. I must have misunderstood my laboratory notes—hasty scrawls. The present account is correct.

sected two of these at once to watch the crescent-sphere-flagella metamorphosis. Nothing else was found, and the metamorphosis occurred in Mosquito 34, just as it did in *Stegomyia* and *Culex*. But I noted the peculiar eggs of the new type and described them as "large and black, boat-shaped with central spines." Here is the rough drawing of one from my notebook, page 106:



EGG OF DAPPLED-WINGED MOSQUITO.

After this I killed the third "malariated" *Anopheles* of 16 August (Mosquito 35), and on dissecting it found some "peculiar vacuolated cells in stomach about 10 in diam." But I added no further note about these cells, and do not remember to have thought much about them at the time. This was a mosquito of the third day after feeding, so that the blood in its stomach had been evacuated; but the dissection was a poor one, and I possessed three more insects of the same batch which I proposed to examine on the morrow—in order to allow the supposed parasites in them to grow as large and as visible as possible. At that time I still credited Manson's idea that mosquitoes feed only once and then die after laying their eggs on the water. I was rather disheartened at my results with the new species, but hoped for better things next day.

The 20 August 1895—the anniversary of which I always call Mosquito Day—was, I think, a cloudy, dull, hot day. I went to hospital at 7 a.m., examined my patients, and attended to official correspondence; but was much annoyed because my men had failed to bring any more larvæ of the dappled-winged mosquitoes, and still more because one of my three remaining Anopheles had died during the night and had swelled up with decay. After a hurried breakfast at the Mess, I returned to dissect the cadaver (Mosquito 36), but found nothing new in it. I then examined a small *Stegomyia*, which happened to have been fed on Husein Khan on the same day (the 16th) —Mosquito 37—which was also negative, of course. At about 1 p.m. I determined to sacrifice the seventh *Anopheles* (*A. stephensi*) of the batch fed on the 16th, Mosquito 38,

although my eyesight was already fatigued. Only one more of the batch remained.

The dissection was excellent, and I went carefully through the tissues, now so familiar to me, searching every micron with the same passion and care as one would search some vast ruined palace for a little hidden treasure. Nothing. No, these new mosquitoes also were going to be a failure: there was something wrong with the theory. But the stomach tissue still remained to be examined—lying there, empty and flaccid, before me on the glass slide, a great white expanse of cells like a large courtyard of flagstones, each one of which must be scrutinised—half an hour's labour at least. I was tired, and what was the use? I must have examined the stomachs of a thousand mosquitoes by this time. But the Angel of Fate fortunately laid his hand on my head; and I had scarcely commenced the search again when I saw a clear and almost perfectly circular outline before me of about 12 microns in diameter. The outline was much too sharp, the cell too small to be an ordinary stomach-cell of a mosquito. I looked a little further. Here was another, and another exactly similar cell.

The afternoon was very hot and overcast; and I remember opening the diaphragm of the sub-stage condenser of the microscope to admit more light and then changing the focus. *In each of these cells there was a cluster of small granules, black as jet* and exactly like the black pigment granules of the *Plasmodium* crescents. As with that pigment, the granules numbered about twelve to sixteen in each cell and became blacker and more visible when more light was admitted through the diaphragm. I laughed, and shouted for the Hospital Assistant—he was away having his siesta. "No, no," I said; "Dame Nature, you are a sorceress, but you don't trick me so easily. The malarial pigment cannot get into the walls of the mosquito's stomach; the flagella have no pigment; you are playing another trick upon me!" I counted twelve of the cells, all of the same size and appearance and all containing exactly the same granules. Then I made rough drawings of nine of the cells on page 107 of my notebook, scribbled my notes, sealed my specimen, went home to tea (about 3 p.m.), and slept solidly for an hour. Here is a facsimile of page 107, exactly as written on that day (Plate V).

When I awoke with mind refreshed my first thought was: Eureka! the problem is solved! I seemed to have found in my sleep an explanation of the pigment. The flagellated spores

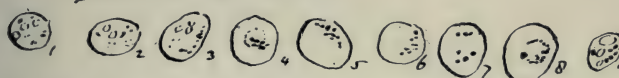
107

20th August 1897

36. Anop. ♂ 16h (4d) dead. Brown with white wings &c.
As usual 5 cells with adhering fat granules ♂ ♀
No pseudo. No filariae

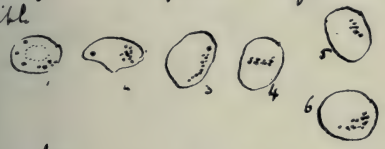
37. Anop. ♂ 16 (4d) dead Small, hindlegs, black
Pseudo ♂♀

38. Anop. ♂ 16h (4d) living Brown with white wings &c
The stomach just under its outer surface contains
some large cells with pigment (? ♀ mummm vacuoles



The pigment sometimes oscillates, is quite black like that of haemo amoeba; ♀ is not found outside these cells. In 8 it is arranged in a circle. The vacuoles do not change position & the cells do not change shape. The outline of the cell is generally clear, but in the smaller ones sometimes delicate. About 12–16μ – diameter.
This specimen injected with ¾0% formalin & sealed with Brehm's glue.

21st August.
yesterday's sealed specimen Pigmented bodies still present, but not more visible



all at rest moving

No 1 shows signs of a nucleus & No 5 & 6 are distinctly more fleshy & bright than yesterday

39. Anop. ♂ 16h (5d day) alive. Large, brown, white wings &c.
The same cells in stomach under superficial layer – one a little larger & better defined

Pigment oscillating in some. Largest about 20μ in diam. Outline much thicker. 2/3 of these in stomach, chiefly towards upper end.

grow in the gastric cells of the Dappled-winged Mosquitoes just as the young *Plasmodia* grow in the human blood-cells, and as they grow they absorb hæmoglobin from the blood in the mosquito's stomach just as the young *Plasmodia* absorb it from the blood-cell (the " pigment " is of course nothing but altered hæmoglobin). I was wrong: my cells were in fact the *female crescents themselves* (Plate II, figs. 22, 23) which had been fertilised by the sperms of the male crescents (which we had called flagellated spores) and were now beginning to grow (ib. figs. 24, 25), *still containing their original pigment*, in the gastric cells of the *Anopheles*. Scientifically they are called Zygotes. But any explanation was enough at the time, and I wrote that evening to my wife: " I have seen something very suspicious in my mosquitoes today and hope it may lead to something." Then I added: " Lately I have been putting together those rhymes I used to make on ' Exile '—you remember. I think I will write them out fair. . . ." But another consideration struck me. If these cells were the parasites they should *grow in size* in the last remaining mosquito during the night; and I spent that night in an agony lest my sole surviving friend should perish and go bad before morning!

Next day I went to hospital intensely excited. The last survivor of the batch fed on the 16th, Mosquito 39, was alive. After looking through yesterday's specimen I slew and dissected it with a shaking hand. *There were the cells again*, twenty-one of them, just as before, *only now much larger*! Mosquito 38, the seventh of the batch fed on the 16th, was killed on the fourth day afterwards, that is, on the 20th. This one was killed on the 21st, the fifth day after feeding, and the cells had grown during the extra day. The cells were therefore parasites, and, as they contained the characteristic malarial pigment, were almost certainly the malaria parasites growing in the mosquito's tissues.

The thing was really done. As I said on page 211, we had to discover two unknown quantities simultaneously—the kind of mosquito which carries the parasite, and the form and position of the parasite within it. We could not find the first without knowing the second, nor the second without knowing the first. By an extremely lucky observation I had now discovered both the unknown quantities at the same moment. The mosquito was the *Anopheles*, and the parasite lives in or on its gastric wall and can be recognised at once by the characteristic pigment. All the work on the subject which has been done since then by me and others during the last

twenty-five years has been mere child's play which anyone could do after the clue was once obtained.

That evening I wrote to my wife: "I have seen something very promising indeed in my new mosquitoes," and I scribbled the following unfinished verses in one of my *In Exile* note-books in pencil:

This day designing God
Hath put into my hand
A wondrous thing. And God
Be praised. At His command,

I have found thy secret deeds
Oh million-murdering Death.

I know that this little thing
A million men will save—
Oh death where is thy sting ?
Thy victory oh grave ?

On the 22nd I wrote to my wife, after mentioning the poem again: "I really think I have done the mosquito theory at all [? last], having found something in mosquitoes fed on malaria patients, exactly like the malaria parasite." Then, or a few days later, I wrote the following amended verses on a separate slip of paper:

This day relenting God
Hath placed within my hand
A wondrous thing; and God
Be praised. At His command,

Seeking His secret deeds
With tears and toiling breath,
I find thy cunning seeds,
O million-murdering Death.

I know this little thing
A myriad men will save.
O Death, where is thy sting ?
Thy victory, O Grave ?

About the same time the two subsequent sonnetelles of *In Exile* were added—also on separate slips for tentative arrangements; and I did not like to change them further when they were published thirteen years later. The three final sonnetelles of *In Exile* had been written previously; and the poem was now finished though I did not know it then.

Reviewing this work after nearly a quarter of a century I still think that no other method but mine would have solved the problem. There was no evidence to incriminate the *Anopheles* by themselves: they are seldom more numerous than other

kinds of mosquitoes, even in the most malarious areas; they often abound only for brief periods; frequently they enter houses only during the night and leave again at daybreak; and by no means all species even of the *Anopheles* carry malaria. It would therefore have been impossible to obtain any genuine statistical evidence of their guilt. So also regarding the parasites in the mosquitoes; there was absolutely no clue to indicate *a priori* their form and position in the insects. There was only one sound method of procedure—the old one of incessant "trial and failure." But this meant an enormous amount of work. It was that extra half-hour's work on 20 August which gave the clue; but even then the success was a miracle of luck. I think that, but for this, the problem would not have been solved today or perhaps even centuries hence.

After my work was published near the end of the year a number of great men of science made the same discovery independently of me; but I always find their claims somewhat unconvincing. I have so great a respect for this class of men that I cannot conceive them capable of the folly of ever undertaking the gamble I was guilty of! I am sure that none of them would ever have embarked on so vast and stormy a sea, would ever have been the Columbus of so wild an adventure, would ever have shown—I will not say the patience, the passion, and the poetry—but the madness required to find that uncharted treasure island! Really they have forgotten what was their true vocation—to stay at home and draw the maps after the event, to colour them red, blue, and yellow, to put their own names to the continents and islands, and to draw their salaries—a much more pleasant occupation.

But to resume—the key of the mystery had been obtained, but the door still remained to be unlocked. I had found zygotes of the fourth and fifth days—but what happened afterwards? Above all, how do the mosquitoes infect men and, possibly, each other?

22 *August* 1897.

Dear Dr. Manson,

In my last I told you that I was going to abandon the brindled mosquitoes for a while, and attack some brown ones. Now prick up your ears because the hunt is up again. It may be a false scent, but it smells promising.

Unfortunately it turned out that my mosquitoes (brown) were very few in number, but they bit well and kept well. I dissected the first ones without any result, but some were bad

dissections, broken eggs, etc. On Friday 20th I had only two left. One was very carefully dissected (4th day after being fed on crescents). I noticed at once some cells in the stomach rather more distinct than the usual very delicate stomach cells. Altogether there were a dozen of these lying mainly towards the upper end of the organ. They varied from 12 to 16 μ in diameter and were full of stationary vacuoles. The outline was sharp but fine and not at all amœboid, the shape spherical or ovoid, the substance rather more solid than that of the neighbouring cells. There was no contractile vesicle ; but one or two of the cells seemed to be contained in some of the large gastric cells. Now I am so familiar with the mosquito's stomach that these bodies struck me at once ; and you may imagine how much more struck I was when, on focusing carefully I found that they contained *pigment indistinguishable in colour, shape, etc., from that of the hæmamœba !* It was black or dark brown (not blue, or yellowish or greenish like the cell granules, debris, etc.), not refractive on up focusing, consisting of little balls or rods, and sometimes oscillating rapidly within a small range in parts of the cells. This pigment was not however copious at all and the granules did not shift their place in the cells and [?] appreciably ; though I find on comparing drawings that they alter their position after 24 hours. In some cells there were pretty large clumps of pigment ; in fact the latter was *exactly* and in *every detail* like the pigment of the hæmamœba.

Now what do you think of this ? Well, I irrigated with formalin, sealed with glue and labelled for you. Examining next day, the cells as I expected they would under formaline, showed more clearly and could now be made out easily with a $\frac{1}{6}$th inch. The pigment had clustered more into clumps. In fact apart from the pigment, the bodies looked parasitic, though they were not nearly so bright as the coccidia I sent you from Sigur.

On the 21st I killed my last brown mosquito (5 days after being malariated) and rushed at the stomach. The very same bodies (!) only larger, more distinct and with a thicker, perhaps double, outline (sometimes). There were 21 of them, again in the œsophageal half of the stomach. The largest ones were 20 μ, but some were only 12–14 μ. Pigment and vacuoles as

before. Formaline, etc., again. I will send you the specimens when I can get some more (I want them now for comparison).

No more brown mosquitoes ! In their absence, I have taken a number of brindled malariated mosquitoes [*Stegomyia*] which I had ready and have examined them again to make sure that I had not previously overlooked the pigmented bodies. Not one in eight of them. Hence we have (provisionally) 1. The pigmented cells appear to exist in malariated brown mosquitoes but not in brindled ones. 2. They are larger on the fifth than on the fourth day. 3. They occupy only one part of the stomach. 4. They become more distinct under formaline. 5. They are sometimes intracellular (?). 6. They have pigment exactly like that of the malaria parasite.

The remarkable thing about them is undoubtedly the pigment. I have never seen this before in any mosquito (I have now examined hundreds—or a thousand). It always lies in the cells and is not scattered outside them or any other cells. In short it is as distinctive a feature of them as it is of the *hæmamœba. No other bodies but these two that I have ever seen contain such pigment : and it is the same exactly in both.* In some cells it even lies in a ring at the centre [drawing]. In others marginally [drawing]. In others, again quite irregularly. In some cells after formaline I think I can see a central nucleus with the pigment round it [drawing]. Wait now. You will say at once (as I used to think) that the parasite in the mosquito cannot have pigment, because this is known to be derived from hæmoglobin, and the mosquito has no hæmoglobin. Has it not though ? Why its stomach is full of it, and these cells which lie in the wall of the stomach, perhaps almost touching its contents, could easily absorb unlimited hæmoglobin. Anyway there's the pigment, just like what we see in our old friend.

Only one difficulty remains—to get more brown mosquitoes. I may add that it is a filaria bearing species. One of my old patients has very rare filariæ and I found one kicking in the *tissues* of a 24-hour mosquito.

I must first keep my mosquitoes alive for more than 5 days, by giving them a second feed, in order to see whether the pigmented cells develop spores. By their appearance on the 5th day, I imagine sporulation will begin at about the 7th day.

If spores form, the parasitic nature of the cells is established and, taken together with the pigment, leads to an obvious inference.

Next I have to see whether unfed brown mosquitoes or the same fed on healthy blood do or do not possess these cells, and whether malariated ones always possess them.

All this is only a matter of a little work. Unfortunately I can't get any more of these brown devils.

Meantime the find of pigment is as encouraging as it is unexpected. It is of course quite unlikely that these cells are mere ordinary cells which have absorbed pigment granules from the stomach contents by amœboid action. The cells are too few and the pigment too scattered and scarce—not like that of a pigmented spleen cell for instance. Besides, why should they not exist in the brindled insects ?

23rd Aug. No brown mosquitoes yet. Can do nothing else but look at my pigmented cells. They keep beautifully in formaline. The pigment, though much more scanty in comparison to the size of the cell, is absolutely identical in character with that of the hæmamœba. I have never seen anything like it before except in the hæmamœba. Wonder if I am really on it at last ! If not, what can these cells be ? Something very unusual.

5. p.m. Two brown mosquitoes [*A. culicifacies*] caught in hospital ward, a male and female, the latter containing one day's blood. No pigmented cells. These then appear to be not a normal feature even in brown mosquitoes.

Two points must be proved : (*a*) that the cells are parasites ; (*b*) that they are malarial parasites. Please note however that owing to the presence of pigment (*a*) will really almost involve (*b*). Hence, if I can find that the cells *sporulate*, the thing is *morally* certain, though formal proof of (*b*) will be required.

24th August. No more brown mosquitoes bred from grubs ; but I have caught 10, 1 male, 7 unfed females and 2 females [*A. culicifacies*] whose stomachs and ova show that they were fed 2 or 3 days ago. In none of these have I found pigmented cells, though all were examined most carefully. It appears then that these cells are peculiar to malariated brown mosquitoes.

With kind regards, believe me,

Yours sincerely,

R. Ross.

There are several important points here. I had hitherto followed Manson in supposing that mosquitoes feed only once and then die three to five days afterwards; but I now found that I could keep them alive much longer by the simple process of feeding them again. For this purpose one had only to apply the test-tube or bottle containing a mosquito to a man's skin. I also inferred rightly that the spores would be produced about the seventh day.

The "brown mosquitoes" mentioned here were of *two* kinds, both "dappled-winged" *Anopheles*. The species in which I had found the zygotes was a large, brown, brindled, and voracious species one of which filled herself in ninety seconds off Husein Khan; but I had obtained all my examples of this species on 16 August and could obtain no others afterwards: and in fact have never seen them alive again; they were probably, but not certainly, *Anopheles stephensi*.

The other species I also found for the first time on 16 August [1] —the single insect which was caught on the wall. But now on the 23rd and 24th they began to abound in the hospital, and later even in my own house a quarter of a mile distant. They were probably *Anopheles culicifacies*, a much smaller, delicate, whitish, plain-coloured insect which exasperated me by refusing to bite. They were soon swarming all over the place without annoying us; and I examined numbers of them caught, fed or unfed, on the hospital walls to see if by any chance some of them contained the "pigmented cells." I find from my notebook that up to 3 September (when I went on leave) I searched at least eighteen of them, with negative results. At the same time I re-examined numbers of *Culex* and *Stegomyia* fed on malarial blood—lest I had overlooked the cells previously —but also without success. The dissection of Mosquito 91 is recorded on 3 September.

SECUNDERABAD, 31*st August*.

DEAR DR. MANSON,

. . . You will be dying to know about the new cells. I have had a maddening exasperating week owing to my not finding any more of those infernal brown mosquitoes; and at

[1] In my Nobel Lecture it is stated that I found this small *Anopheles* early in July or August *before* I found the large one. This error was partly due to the fact that my letter to Manson of 18 August was wrongly dated 18 July by a slip of the pen—as I have just ascertained. I first found both species on the same date, 16 August, when they evidently began to hatch out after the heavy rain of 16 July.

the end of this week I must go to Bangalore to see my wife and children. Is it not annoying ? [1] I have had half a dozen men out hunting grubs and have obtained scores of batches—all brindled, or grey. The ones I caught in the ward turn out not to be exactly the right kind, though very closely allied, being whiter and more slender [*A. culicifacies*]. I cannot find where even these come from, and those caught by hand won't bite, even at night. These kinds constitute evidently a separate family of mosquitoes characterised by having dappled wings and peculiar boat-shaped eggs. I will be on them in time.

I have examined crowds of other kinds for the pigmented cells, without result. I am now going methodically through a grey species with six dark grey bands on back and grey plain wings [*Culex fatigans*]. Will let you know result up to 4th day tomorrow.

Now to open my heart freely—I really believe the problem is solved, though I don't like to say so. I look at my cells daily ; those of the fifth day have grown bigger than those of the fourth day ; such bodies are not found normally along the elementary cells of the mosquito's stomach (which are alike in all species of course) ; they have as yet been found only in malariated mosquitoes ; above all, they contain the characteristic pigment peculiar to the malaria bug. What are we to think : What do *you* think ? I fancy when you first read the word " pigment " you guessed what it was. Well, now, I dare'nt send you these specimens for fear they spoil on the way. I will take them to Bangalore and show them all round. John Smyth is there I think and some other men who have got a little knowledge of the malaria bug. I'll make them write descriptions, which I will send with a short note of my own to the *B.M.J.* . . . My note to the *B.M.J.* will contain description of the mosquito and of the cells and will draw only such cautious references as we are as yet entitled to make. In reality —*pigment*—it is almost proof already ! What else can the thing be ? Mosquito's cells don't contain pigment or anything like it. I am not mistaken—it is *pigment*, absolute, unmistakable *pigment* ! I have hardly restrained myself from wiring

[1] I hope my wife and children will not observe this. The interrogatory objurgation refers to the following, not to the previous, sentence.

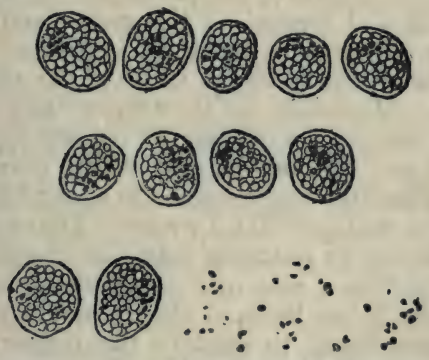
" pigment " to you, but fear you would think I had gone mad. Well, I know pigment by this time. *I am on it.* . . .

With kind regards,

Yours truly,

R. Ross.

On 2 September 1897 my wife and children left Ootacamund for Bangalore, where they stayed at the West End Hotel ; and I joined them there on 4 September on ten days station-leave. My first care was to write a paper [29] on my recent discovery for publication, called " On some Peculiar Pigmented Cells found in two Mosquitoes fed on Malarial Blood " ; with note by Surgeon-Major Smyth, M.D., I.M.S. I had brought my two specimens with me and begged my friend, John Smyth, a very capable civil surgeon of Madras, who was then in Bangalore, to verify my finding. The article was despatched to the editor of *The British Medical Journal* between 4 and 13 September, and finally appeared (undated) in the *Journal* of 18 December 1897, with Smyth's note and with excellent drawings of eleven of the pigmented cells made under Manson's direction, and with additional remarks by Manson, Dr. Bland Sutton, and Dr. Thin (page 253). Here is a copy of the drawings.



DRAWINGS OF " PIGMENTED CELLS " MADE UNDER MANSON'S DIRECTION FROM ROSS'S SPECIMENS AND PUBLISHED IN " THE BRITISH MEDICAL JOURNAL," 18 DECEMBER 1897.

It is important to note that I also described the large dapple-winged mosquitoes as best I could, saying that : " The wings are light brown to white and have four dark spots on the anterior nervure. . . . The eggs—at least when not fully developed—are shaped curiously like ancient boats with raised stern and prow, and have lines radiating from the concave

border like banks of oars." Compare drawing, page 223. This description enabled the Italians to recognise the genus (*Anopheles*) next year (Chapter XVIII), as I had hoped they and others would do.

At the same time I wrote a full official report of my find to the Director-General of my service and forwarded it through the proper official channel on 19 September, after my return to Secunderabad on the 15th; that is, this report went directly through the Principal Medical Officer, Secunderabad, Dr. Major, and the Military Surgeon-General at Madras, Dr. McVittie. At the beginning of it I state that "The cells are in my opinion very probably the long-sought alternative form of the parasite of malaria in the mosquito"—so that these gentlemen must at least have known that I *thought* I had at last found something of some trifling importance, and they should perhaps have tried to help me a little.

On 13 September I sent Manson my two specimens ("worth £1,000 each") with maps and full description. On return to Secunderabad on the 15th I was intensely disappointed to find that my men had secured no more of the large *Anopheles A.* (*stephensi*), and therefore started out myself to look for the larvæ, hunting every pot, puddle, and pool for them. On the 21st I found hundreds of the characteristic larvæ in a rain-water pool which had formed on the grass—and subsequently in similar pools and *only* in them, an important observation; but these all turned out to be the larvæ of the small *Anopheles* (*A. culicifacies*). Meantime, on the 18th we had caught an old *Culex fatigans* feeding on a case of spring tertian malaria (*Plasmodium vivax*), and on the 21st it was found full of pigmented cells. This find was described in a long letter to Manson of the same date; and next day I wrote: "I have just succeeded with great difficulty in inducing four[1] of the allied dappled-winged species to bite a case with numerous crescents. These little wretches die if one looks at them; but if they live until the 25th and are found to contain the pigmented cells the theory will be practically proved." These were, of course, the small *Anopheles*, and they had all been bred from the larvæ which I had found in the pool of rain-water on the 21st. Three of them died in the next two days; and one of these which I dissected was negative. On the 24th, fearing that the single survivor would die also, I dissected it at once two days after it had been fed. It contained a number of pigmented

[1] The account in my Nobel Lecture is again not quite accurate; four, not two, had fed themselves.

cells, all of the same size and very small—about 8 μ, the size of a red corpuscle and of an ordinary crescent-sphere before it emits its "flagella."

I had therefore now obtained a series of four mosquitoes in which the pigmented cells increased in size according to the intervals which had elapsed after the containing insects had ingested malarial blood. The diameters of the cells were 6 μ to 8 μ at two days; 12 μ to 16 μ at four days; up to 20 μ at five days; and up to 25 μ in the *Culex fatigans*. The periods of the three first mosquitoes, as arranged here, were certain, because they had all been bred from the larvæ in bottles and had been fed at known times; but the period of the fourth was not certain, as it only had been caught by hand, but I calculated that it may have been feeding for many days and was at least a week old (pages 236, 412).

I now had numbers of the small *Anopheles*, all bred in bottles from the larvæ, of which crowds could easily be obtained in neighbouring puddles; and I had also found that adults hatched from larvæ in captivity bit more readily than those caught wild. There were many cases of malaria, both *P. falciparum* and *P. vivax* in the ward, and I had a number of well-trained assistants and willing patients; so that I calculated that in a week or two I should discover the whole life-cycle of the malaria parasites, including the mode of infection. On 22 September I had written to Manson: "I shall be much disappointed if I don't get practical proof in a week's time. . . . A telegram to you will mean this definite announcement, and when you get it you may know that I am absolutely sure." On the same date I wrote to my wife: "I told you in my post card that I had found those pigmented cells again, and yesterday I found also some of the right kind, or very nearly the right kind, of mosquitoes. Today I persuaded four of them to bite a patient, I discover that this kind grows only in rain-water puddles. If these four contain the pigmented cells I think the theory will be proved. Anyway I expect to prove it completely in a fortnight or so and to telegraph home to Manson. What a fuss there will be!" On the 24th I wrote to my wife: "No more work on the germs, but I shall be disappointed if I don't finish in a fortnight. It *will* be a great thing. . . ."

But the blow now fell; fate willed otherwise; and my next letter to Manson was:

BOMBAY, *September* 27, 1897.

DEAR DR. MANSON,

I fear I must stop my malaria work at this point as I

have been ordered to the front on active service. This, so far as my share in the work is concerned is unfortunate as, when I received my orders, I hoped to complete the mosquito theory in a week or so, and now I think it very unlikely that I shall be able to resume investigation for many months, if at all. Perhaps therefore I should give you a winding-up statement of my studies since I first met with the pigmented cells.

Since the 20th, when I first saw them, I have not been able to find the large brown dappled-winged mosquitoes [*A. stephensi*] again. On the 18th Sept. however I saw a grey mosquito [*C. fatigans*] (legs and wings plain, belly nearly white, back of abdomen barred dark-brown) feeding, on a patient whose blood the microscope had shown to contain the mild tertian parasites. Numbers of similar mosquitoes to these, unfed or fed on blood containing crescents, had been previously examined by me and found not to contain pigmented cells. This one was secured. She was an old insect to judge by her ovaries and had, I thought, often fed on this particular patient who slept in a corner of the ward. I kept her alive for three days more and then examined her. Her stomach contained numerous pigmented cells varying from about 10 μ to 25 μ in diameter (I have no micrometer), most of them like those formerly found in the brown mosquitoes, except that the pigment appeared to be finer, scantier and browner, while the cells differed much in size and some of them were very large. A few of the largest cells however differed markedly from the others, the substance being apparently free of vacuoles and consisting of a hyaline substance in a capsule. In the centre there was a nucleus (or vacuole ?) on the surface (?) of which the pigment was scattered. In the smallest cells the pigment was often oscillating just as in some forms of the hæmamœba ; but in none of them did I notice any amœboid movement.

The discovery of the cells in this mosquito immediately suggested of course that the grey mosquitoes with barred backs, though they appear immune to the summer-autumn parasite, are susceptible to the mild tertian. Proof appeared easy to obtain, by feeding numbers of this species of insect on mild tertian blood and dissecting them after some days. Unfortunately I was not able to do this until the day before I left Secunderabad, and the insects have all died in the train.

I was however a little more lucky with some of the delicate, brownish-white, dappled-winged insects [*A. culicifacies*] which I have already referred to as being closely allied to those in which I first found the pigmented cells. I obtained the grubs of these with great difficulty (they appear to breed only in rain-puddles) and fed the imagos on crescent-blood. They are wretched little creatures and die at once almost, but I managed to examine two, 24 hours after feeding, and one, 48 hours after. In the former I could find nothing; in the latter there were a number of pigmented cells, all nearly the same size and very small, only about 8 μ in its long diameter, containing very marked and, for the size of the cell, copious pigment which sometimes oscillated and was sometimes arranged in the form of a ring. The cells themselves were so hyaline that they were scarcely visible; but there could be no doubt about the pigment which was typical of the malaria parasite. I had no time to experiment with any more of this species.

This then closes my list of findings of pigmented cells, namely twice in large dappled-winged mosquitoes fed on crescent-blood, one in a small dappled-winged mosquito fed on crescent-blood, and once in a barred-back, plain-winged and legged insect fed on mild tertian blood. Of the small dappled-winged insects I have examined numbers, fed on healthy blood or unfed, without finding the cells; of the barred-back insects I have dissected as many unfed or fed on crescent blood, without seeing the cells. It would appear then as if these pigmented cells are the direct result of feeding appropriate insects on appropriate kinds of malarial blood. . . . In the barred-back mosquito containing pigmented cells (she must have been quite a week old) the stomach contained three large clusters (40 or 50) of swarm spores, each cluster breaking up rapidly under the influence of water into a multitude of these swarm-spores or flagellulas, and each flagellula almost as large as a trypanosome. All the latter then rush about in the cavity of the stomach with the ordinary movements of flagellulas. I have seen these parasites before in the malarious locality of the Sigur Ghat. The question now arises whether they are not the spores of the pigmented cells, the homologues of the ordinary flagella arising from crescents, etc., but which, in this case,

continue the life of the parasite in external nature, or perhaps are deposited on the human skin.

I must now conclude; and shall do so by laying great stress on the presence of the so-called pigment in the cells. This is a substance not found in ordinary physiological cells; or in ordinary protozoa; or in gregarines and coccidia; or even in the gregariniform blood parasites of reptiles; it is seen only in the hæmamœba of men and birds. What is to be said then when we now observe it in mosquitoes actually fed on blood containing hæmamœbæ?

Yours sincerely,

R. Ross.

Probably the *Culex fatigans* had not been infected from the man with tertian malaria at all, but, some days previously, from a bird containing the bird's malaria-parasite, then called *Proteosoma relicta*—which proved next year to be carried by this species of mosquito. As stated above, the same insect also contained numbers of the flagellate parasites I had found in Sigur, later called Crithidia Léger, and a man to whom I gave them had fever afterwards (page 211). I therefore conjectured, reasonably enough, that these might be the spores of the zygotes—I was wrong here. The name *Hœmamœba* was then often employed instead of *Plasmodium*.

What had happened was this. As stated above, in June 1897 the Director-General had offered me an appointment either in the Central Provinces or under the Indian Foreign Office, and had later told me that he was going to give me more favourable opportunities for my investigations (page 216). If any one of these promises had been fulfilled (as I had daily been expecting them to be), I would probably have been given plenty of time to wind up my self-imposed task at Secunderabad in a proper manner and to collect my mosquitoes for identification before going elsewhere. But there was another possible development. The tribes on the Indian frontier had been giving much trouble and the Tirah campaign against the Afridis had begun; so that, as I had warned Manson, I might be ordered away on active service at any time and at a moment's notice—a thing that would ruin my work just at the point of success. It was not my turn to go, but that of my racing friend (page 202), who was now training horses at Secunderabad; so that I was not very apprehensive that such a calamity would befall me. But on 24 September,

just after I had found the pigmented cells in the last mosquito and was gloating over this proof of the theory, the cruel Angel of Fate, who had been so kind to me before, now struck me down with the following order:

"Under instructions from Command Headquarters, Surgeon-Major R. Ross, I.M.S., will proceed immediately to Bombay for military duty."

This did not necessarily mean "active service" (which I might have enjoyed for itself), but was equally disastrous to my work. I examined some more mosquitoes and then, leaving my family at Bangalore, departed on 26 September from Secunderabad—I suppose, for ever.

No sooner had I found the Treasure Island, than I was driven away from it by an unopposable gale. I saw the Promised Land, but was not allowed to enter it. Owing to no fault of mine, two long years were to elapse before I was to see again—in another continent—that wonderful revelation of human malaria in mosquitoes. During that time all my work was to be pirated by foreigners; and the same maladministration which now drove me into the wilderness was to force me finally out of India just when my discovery might have been of real help to her swarming and dying millions.

CHAPTER XIV

PUNISHMENT. KHERWARA, 1897–1898

By whom and for what purpose that order was originally issued we have never been able to ascertain. Although I had just reported my new and astonishing results to my official superiors, the order was not accompanied by any intimation, official or private, as to where I was likely to be sent, what I was to do, and how long I should probably be kept away from my work. It was not even stated whether I was likely to be dispatched on active service—in which case only eighty pounds of baggage were allowed, not enough for my microscopes. The order came from the military Surgeon-General at Madras (Dr. McVittie), and I therefore wired privately to his secretary, whom I knew, asking for details. The answer was, "Do not know, possibly to accompany field hospital." I wired also to the Principal Medical Officer, Bombay, who replied: "Nothing known here about you." I was so stunned by the misfortune that I remember little of what occurred at the time; but believe that my Principal Medical Officer, Surgeon-Colonel Major, was away and that I went to the acting Commandant of my regiment and begged him to try to get me delayed for a week or two—he was a young man, much interested in my work (many more laymen than doctors were interested in it)—but could do nothing. I seem to have heard somewhere that the order had come originally from Simla, and that I was to be employed at Bombay on important scientific or sanitary work, and therefore wrote much more cheery letters of departure to my wife than I would otherwise have done. But I was next medically examined as to physical fitness for active service; so that I left Secunderabad early in the morning of 26 September 1897 quite befogged as to my future and with the smallest possible "kit"—but with my microscope and my faithful Lutchman.

On arrival next day at Bombay I called at once on the local P.M.O., who was really sympathetic about my malaria work, but did not know why I had been sent to him. He

expected it was for active service, but telegraphed to his headquarters for instructions. Next morning this good man—whose name I regret I do not remember—actually came into my room at the hotel at 7 a.m. and told me with pleasure that I should be able to continue my malaria work because (as I wrote to my wife) "I was to go not to the front but to Rajputana. I don't know exactly what the appointment is. It seems to be half military and half Residency Surgeon. I arrive there tomorrow evening. The appointment is in the much coveted Rajputana Medical Service. I believe the climate will be splendid in a few weeks. Mount Abu is quite close (the hill station). . . . I hope you and the children will be able to come to me in a few weeks." To Manson I wrote the same day: "I shall get my Residency Surgeoncy, I believe, at last. . . . Hope to start work again immediately on arrival." The place was Kherwara. In Rajputana! Think of it: the country in which Dobson and I built our Spanish castles; my long-coveted Island of Barataria!

But when I went to the Adjutant-General's office for my railway warrant the man there did not know where this delectable place was, but found by the map that it was in the centre of a large circle of railway lines, so that a long road-journey from some point on the circle was required to reach it. He did not know where this point was, but guessed that it might be a place called Vadnugger, near Mehsana, a long way west of Kherwara. Off we went therefore on the 28 September; but on arrival next day at Mehsana I found that no transport was available from Vadnugger, and that I must go by train farther round the circle to Rohera in order to get it. There I was laughed at and told that I must go to Oodeypur (Udaipur), half round the circle via Ajmir and about 400 miles away, from which I should have to proceed to Kherwara by road. I was much cheered *en route* by a happy young fellow-traveller, who was going, "seriously ill," to Mount Abu, and who told me that Kherwara was the "unhealthiest hole in India" and was called the "convict-settlement" because only officers in disgrace were ever sent there! Of course I had to pay for all the extra journey from Mehsana out of my pocket, and did not recover the money for many months, and then only after much correspondence. On arrival at Oodeypur, on 1 October, however, I was carried off to the Residency by very kind and delightful friends, Major and Mrs. Ravenshaw. He had been Assistant Resident at Bangalore while I was there and was now Resident at Oodeypur; and, as I said, they fed me

as the ravens fed Elijah in the wilderness—and I needed it, because I was ill and sleepless with disappointment, not ameliorated by constant toothache! Here I received orders not to proceed—followed by orders to proceed; and at last reached Kherwara, after a tonga journey of fifty-six miles, on 7 October. It was a very small station, not even possessing a telegraph office, isolated in the midst of miles of wild country, and the headquarters of a local regiment, the Bhil Corps. The only Europeans there were the Commandant, Major R. A. Cole, his wife, and three children, the Adjutant, the Medical Officer, and, sometimes, a missionary. I called on Major Cole at once and asked him about the Residency Surgeoncy. He laughed at the idea—the only thing in support of it was that the place had been under the Indian Foreign Office up to a year previously. I then said that I could at least study the malaria. He laughed again and told me that there was none there—there had not been a case in hospital for two months! In fact I had been sent all this way, about 1,100 miles, merely because the medical officer, a young man of no experience, had reported sick with a small complaint which he did not know how to treat, and had asked for sick leave to England on the strength of it. There was no illness in the place, and his medical duties could have been easily done temporarily by an assistant-surgeon, or even by a hospital assistant. Was it for this that my malaria work was interrupted?

The ostensible cause for the transfer was that the Bombay medical service had been depleted by the Tirah campaign and that the Madras medical service had to help to make up the deficiency. Now I am very strong on the point that all private consideration must be suppressed in the face of public duty. But my work on malaria was not a private consideration, but a matter concerned with the lives of millions of people. There were many junior officers in Madras who were not doing, and had never done, work of any importance whatever, and there was my racing friend already alluded to. Why particularly was I selected for this petty job? The Surgeon-General of Madras (Dr. McVittie) must have issued the order a day or two after receiving my report on the finding of the pigmented cells.[1] I had then been working on the subject for two and a half years; numerous papers had appeared on it, and I was supported by Dr. Manson and the medical Press.

[1] I *suspect* that when the report reached him he said, "Here is this fool writing about mosquitoes again: let us give him a change of scene." I hope there may be someone still alive who can authoritatively deny this.

If he had disbelieved in my work why did he not question me about it, come to see my specimens, send for me to explain myself, or get someone more competent than himself to make inquiries? If he believed in it why did he not help me? Hitherto my work had not cost the Government of India one anna. But nevertheless, instead of helping me, he now broke up my investigations; and, what is more, he must have known that he was doing so. And the civil Surgeon-General at Madras (Dr. Sibthorpe) was not free from blame, because he might easily have helped me in many ways all this time if he had thought fit to do so.[1] Lastly, the Director-General at Simla (Dr. Cleghorn) should have taken stronger steps to redeem his promise made to me in June; but, to do him justice, I think that he tried to remedy the mistake as soon as he heard of it a few weeks later.

What most alarmed me was the experience of Surgeon-Colonel W. G. King. In 1890 he was able greatly to improve the smallpox vaccine used by the Madras Government; but the Sanitary Commissioner of the time was not favourable. King was deprived of his appointment and was then sent about, at short intervals, from pillar to post in India and Burma as a punishment, and was nearly ruined in health and pocket. Finally, however, the matter appeared in Parliament and Press at home, with the result that the said Sanitary Commissioner was "allowed to resign" and King was given the appointment in his place (*British Medical Journal*, 1892, 1894; *Lancet*, 1893). Naturally I thought that I was now perhaps to be subjected to a similar course of correction.

It has often been suggested that I was the victim of someone's personal animosity, or of jealousy—as Surgeon-Colonel W. G. King had been, or as Mr. Haffkine was to be some years later (page 487); but I scarcely think so. Thus on 28 July I had written to Manson in reply to a question of his: "No; I don't think I have a single enemy in the country. Sibthorpe doesn't love me quite I believe—or else his manner is a very bad one—and ———— who travelled lately with Leslie tells me that the latter did not think much of my work, malarial or sanitary. . . . A Madras man said to me, 'We I.M.S. men are not meant for research and so on—we are simply doctors.' There is a certain feeling against not only research, but men whose names are seen in print." The silly

[1] Dr. R. H. Elliot, who was then his secretary, tells me that the opposition did not come from him but from the military side and from Simla, because the medical bigwigs there were still fast rooted in the opinion that malaria was due to a marsh miasm.

pose of the doctor who is above science might have been partly responsible; but the fact is that the heads of the medical services did not fully perform their duties. These expensive organisations were not made to provide sinecures for sporting gentlemen, or for the Do-nothings and Think-nots of the profession, but to cure and to prevent disease by the best means possible. For this purpose incessant research was and is required, and the heads of the services should have seen to it that every new discovery or suggestion was followed up immediately. At least they might have protected voluntary workers against unnecessary interruption and have advanced them, when they obtained good results, in preference to the scores of people of the other class who so easily obtained preferment to the best posts in the country.

Of course I was quite crushed by the disappointment, and to the present day cannot remember it without indignation. For four months I was imprisoned in that place. During that time I could easily have worked out the whole life-cycle of the human *Plasmodia* in *Anopheles*, have discovered the mode of infection, and have infected healthy men (beginning with myself), just as I did with birds' malaria a year later; and I could have incriminated one, and perhaps two or more species of *Anopheles*, and have shown how to reduce them—none of which things were done in India, owing to the interruption, until years afterwards. Meantime the people of India were dying at the rate of over a million a year from malaria. The man who was officially responsible for this disaster was, I suppose, Dr. McVittie, the military Surgeon General at Madras.

I made several desperate efforts to escape. On 2 October I wired from Oodeypur to the P.M.O., Bombay District, telling him that I had been ordered not to proceed to Kherwara and adding: "Beg you will telegraph this my application to be allowed if possible to return to my regiment or send somewhere where I can finish mosquito malaria theory now on point of completion as lately reported to Director-General I.M.S." On the 17th I wrote a humble letter to the same effect from Kherwara, but received only the following reply from that august being, Surgeon-Major-General Illegible, P.M.O Bombay Command (the head of the Bombay military medical service):

"I don't understand what this officer means. He was sent to this Command by H.E. the Commander-in-Chief and here he will remain until H.E. orders him away."

I had already written privately to Mr. J. P. Hewett, Secretary in the Home Department at Simla, to whom I had a letter of introduction from Sir Charles Crosthwaite of the India Office (through Dr. Manson), asking whether the proposed malaria investigation by the Government of India was likely to be soon commenced, and his reply in the negative reached me when I arrived at Kherwara. So that hope was gone. On 2 November therefore I applied for the post of Residency Surgeon at Rewa; it was refused me. I then asked for a better paid regiment than my own, namely, the Madras Sappers and Miners at Bangalore, with which I had served in 1884. Surgeon-General McVittie rejected my application. Meanwhile some of the Indian Princes had long been forming a new Health Institute in northern India for research, and I applied through Surgeon-Major Owen of Patiala (page 162) for a post in it; but on 2 December I was obliged to send Manson the following post-registered telegram:

"Health Institute refuses apply for my services advise you make one last effort on behalf of theory and myself if not almost certainly leave India next March if possible wire result."

On 11 December therefore I asked for a month's leave to go to Madras to look after my family and then to Calcutta to represent the position of the malaria work to the Government of India: the leave was refused. In fact I was imprisoned at Kherwara as securely as ever Dreyfus was in the Île du Diable.

Apart from the interruption of the malaria work my personal affairs were not in a wholesome condition. Owing to frequent changes of station and to the necessity of keeping my family in one place and myself in another, we had been obliged to spend between 3,000 and 4,000 rupees above my salary, and there was no prospect of advancement in the service, whatever work I did. To remain in such a position was not a "business proposition," and I decided to leave the service on the first opportunity. At the same time I wished (and intended) to finish the malaria work, and it was just possible that Dr. Cleghorn might put me on special duty for the purpose before my pension was due to me (23 June 1898). Or I might be sent back to Secunderabad, or might even be able to continue the malaria work at Kherwara itself in the spring. Hence I did not send in my papers at once; but in my application for one month's leave I intimated that I should have to do so.

Apart from these tribulations Kherwara would have been pleasant enough to me—a pretty little station in the midst of miniature mountains and valleys which when I arrived were

covered with grass and small woods and possessed a charming little stream in which Major Cole's boys and I caught numbers of small fish like dace with the fly—they contained trypanosomes. Cole himself was a capable, kindly man; Mrs. Cole was a sympathetic and hospitable lady, and their large rambling bungalow was known as the "Hotel Cole." They were extremely kind to me. Even in October the climate was so cool that mosquitoes, including a few *Anopheles* derived from larvæ found in a pit, would not bite, so that apart from the absence of malaria cases my work was paralysed; and later it became even cold and foggy at night. At first we had some tennis and shooting; but the adjutant and doctor soon left and for two months I was obliged to mess by myself and lived the life of a hermit. Every night a monkey wandered round my solitary bungalow making a moaning sound in the mist, like that attributed to an Irish banshee. Soon the grass and the woods withered away and my stream dried up and vanished utterly like a scene in fairyland, leaving only a desert behind. At first I could do no work at all, but spent my time writing letters and redrafting parts of my manuscript novel *The Revels of Orsera*; but in December began some research on the malaria of pigeons. My article on "Pigmented Cells" [29] (page 233) was not published in *The British Medical Journal* till 18 December 1897, and did not reach me till 8 January 1898. The following extracts from my letters to Manson will indicate the progress of events in regard to "The Theory."

On 5 October 1897 I wrote:

"I now write to urge you to exert all your influence to get the Indian Government to take up the malaria question directly the Tirah expedition breaks up (which ought to be by Xmas). Since the subject was last mooted to Government the pigmented cells have been found; and it seems to me that it would be insane on the part of the Government of a large tropical country like this to hesitate any longer in taking up the matter, especially as the thing can be arranged at once free of cost. Of course the Government will say they have no funds after recent events, but this is no impediment. You remember that the Maharaj Rana of Dholpur and other Indian Princes have come forward with funds for a Health Institute. If the Indian Government could see its way to mooting the point to them, I believe they would subscribe at once to pay me while on special malaria duty. This would practically start the Institute immediately on its

protozoal side, for which a complex building and laboratory are not essential."

On 12 October I wrote:

"On examining again to-day my third mosquito with pigmented cells found three weeks ago I observe that the formalin has made the pigment turn into clusters of black crystals which make it appear different to what it was at first and much more bulky and distinct. * * * the doctor here, says the crystals look like hæmatin ones. I tell you this to warn you that the pigment of the specimens which I sent you may have undergone a similar change; so don't be surprised if their pigment differs from the descriptions of Smyth and myself and from that of the malaria parasite in man. I fancy formalin has some chemical action on pigment. My fourth mosquito does not show this change.

"You will be disappointed to hear that there is *no fever* here and I cannot go on with the work at present. . . . I enclose Mr. Hewitt's reply to my letter asking when if ever Government would take up the malaria subject. You observe the vast amount of interest he takes in the matter; and I can only assure you again that there is scarcely a man in the Government or in the Department that really cares one button for all the science in the world. Many men who are doing nothing might for instance have been sent on this wild-goose chase instead of me."

In his letter to me of 22 July 1897 Manson told me that he had been appointed Medical Adviser to the Colonial Office; and, next day, that the Secretary of State for India had written to the Viceroy about my being appointed for malaria work. His textbook on *Tropical Diseases* was finished before 11 August; and on 1 October he acknowledged receiving my letters announcing the discovery of the "pigmented cells," suggested that I might have applied chemical tests to the pigment, and wondered whether the pigment might not have been merely derived from the blood in the mosquito's stomach. *The British Medical Journal* had sent him my paper. Next he wrote to me:

21 Queen Anne Street, Cavendish Square,
21 *October* 1897.

My Dear Ross,

I have been so oppressed with work lately that I have not found time to answer your last two or three letters which,

I need not say interested me acutely. You seem to be on it at last. The slides arrived a fortnight ago while I was away in Scotland on my week's holiday. I examined them at once on my return and found them in good condition enough to recognise the cells you describe. But the preservative medium was evaporating and fearing the cells might perish I had them drawn at once by a good artist and I hope they will appear soon in the *B.M.J.* along with your paper. I showed them to Thin and to Bland Sutton who at once recognised their likeness to the malaria parasite and the importance of your find. Personally I believe you are on the road to great discovery, but first you must be sure of where you are. Two things occur to me. The pigment in the cells may be absorbed by these bodies from free pigment in the mosquito's stomach; second, the pigment may represent an entire malaria parasite which has been bodily incorporated from the blood by the stomach cell. In either case it would not represent a phase in malaria evolution of course. These possibilities you must try to get rid of if you would choke off carpers.[1] I asked the editor of the *B.M.J.* to request Thin and Bland Sutton to make a brief statement to the effect that they had seen your slides and that they consider your descriptions were correct. A similar statement I have made myself and I suppose these corroborative statements will appear as an appendix to your paper. I told them to hurry up with the publication. . . . I hope you have got a plentiful supply of mosquitos and malaria cases, and that you are now thoroughly settled down and at work again. I quite expect that ere the winter is over you will have solved the business at all events in its main features, and that it will have been conducted so far by you that a crowd of workers will be on the details. Dont worry over them. Establish the principle and leave details to others; meanwhile move on yourself to some other big subject.

Yours sincerely,

(signed) PATRICK MANSON.

KHERWARA, RAJPUTANA, 10 *Nov.* 1897.

DEAR DR. MANSON,

I was greatly relieved at getting yours of the 21st Oct.

[1] They were both *impossibilities*. If not, the same thing should have occurred in every "malariated" mosquito I had examined.

with enclosures and at hearing that the specimens had arrived all right. All I hope is that the pigment in the cells has not undergone crystallisation under formalin as in one of the specimens with me. The pigment ought to be in dots, not in rods; like this [drawing] not like this [drawing]. I did not apply tests for pigment because it would not do to destroy my first invaluable specimens. This and staining for nuclei, etc., can easily be carried out afterwards. The crystallisation of the pigment is however a test itself, because I note that the formalin has had precisely the same action on a small residue of blood remaining in the stomach, that is, has converted it into minute black hæmatin crystals just like those in the cells.

Now for the view that the cells may be ordinary stomach cells which have absorbed and converted hæmoglobin or which have incorporated entire parasites. As a matter of fact this is what I thought they might be when I first saw them; but a little reflection shewed it to be extremely unlikely. If the cells be such they must be *physiological* cells, that is, *ordinary* cells of the mosquito's stomach capable of such action and should therefore be found in all, or in a large percentage at least, of mosquitoes. Such is distinctly not the case. I have *never* seen them in hundreds of brindled malariated mosquitoes; nor in scores of barred-back mosquitoes fed on crescents or healthy blood; nor in numerous small dappled-winged mosquitoes fed on healthy blood. But I have found them in both of two large dappled-winged insects fed on crescents, in one barred-back insect fed on tertian blood, and in one of two *small* dappled-winged mosquitoes fed on crescents. This rarity of the cells precludes their being physiological and the objection is practically dealt with briefly in my paper already when I argue that the cells cannot be physiological for this reason. The thing comes under the obvious law that the constantly-found cell is physiological, the exceptional cell parasitic, or at least pathological. If these cells are as you suggest why are they not found in all fed mosquitoes? Besides, *do* the stomach cells take up solid particles from the stomach? On the second day of digestion the stomach is full of solid yellow particles, but I have never seen any such in the stomach cells. If the stomach cells can incorporate whole malaria parasites, why can't they incorporate whole blood corpuscles or other objects?

Lastly, my pigmented cells shew distinct evidences of growth in relation to the time after ingestion. They were small in the mosquito at 2 days, much larger in that at 4 days, still larger in that at 5 days, and largest of all and in company with large clusters of flagellulæ in the mosquito at one week or so. Stomach phagocytes would not have this history. As a rule the largest physiological cells are phagocytic, so that in the mosquito at the 2nd day large instead of small pigmented cells should have been found. Then, are stomach cells ever known to be phagocytic? If so we should expect to find numbers of them filled with débris of all kinds. No, I don't think the pigmented cells are physiological, but, as I said at first, the point requires formal proof. A month's work would give it. All the same the betting is, even now, thousands to one that the cells are the malaria parasite in the mosquito.

Alas for your hope that I shall have a plentiful supply of mosquitoes and cases here! None of either (or very few); the cold weather is in, and the people here (Bhils) so savage and superstitious that they won't allow experiments to be made on them. I had a very slight case of tertian two days ago, and some small dappled-winged insects. The fool flatly refused experiments. I don't think the insects would have bitten though, it is too cold. People sleep here without mosquito nets.

I was quite knocked over by disappointment anent the theory when I got here, and suffered from sleeplessness and neuralgia for weeks, but am better after a course of fly-fishing. You must confess that it is trying to be snatched away just as one is on the point of proving a theory which one has been thinking of and working at for nearly three years; let alone being sent to a wretched place with two or three Europeans and a few itch cases and febriculæ in hospital when one has every right to expect something fairly good. . . .

Yours sincerely,
R. Ross.

On 17 November I wrote:

"Another reason why the pigmented cells cannot be phagocytes is that they all contain the same number approximately of grains of pigment. If they were phagocytes some would contain a large amount of pigment, others a small amount—

compare parasites with pigmented phagocytes in malarial blood.

"Many thanks for Laveran's and Simond's books, which I have received. Marchoux of Senegal has also sent me his brochure. . . .

"By the way it is sad to see that Laveran still sticks to that folly of the unity of the parasite. Marchoux also adopts it, and Metchnikoff has gone in for it, apparently on Laveran's persuasion. What a curious argument Laveran uses. He admits that different forms of the parasite are found, but says that the difference is due to climatic and similar influences. Marchoux says it is due to the resistance of some patients being greater than that of others. Not only is this quite unlikely and about as much as to say that a horse will change into a donkey if fed on thistles and kept in the proper kind of paddock, but won't explain facts. I, for instance, have seen [that] at Begumpett we have tertians and remittents; close by at Hyderabad there are *quartans* and remittents. At Madras there are nearly all quartans; at Bangalore nearly all remittents with a few tertians from one locality. Fresh infections always (with the exception of rare cases of mixed parasites) show one or other of the various species. The thing is so marked out here that I personally have no doubt of plurality of species. What a lot I scribble to you! Absolutely nothing else to do. . . ."

The paper by Simond, *L'évolution des Sporozoaires du genre Coccidium* [27], was very important. The Coccidia of rabbits have "flagellate forms" very like those of malaria, and Simond suggested that the so-called flagella are really sperms (page 197). I doubted this at the moment.

On 6 and 9 December I wrote Manson long letters regarding the telegram which I had sent him on the 2nd, and definitely stated that I must leave the service if not placed on special duty for a year.

On 14 December I wrote:

"I have of course been making every effort to get to work here. I fear that this is the most impossible place in India for it. . . . It is no go, in fact. I have however been examining bloods of frogs, toads, fish and numerous birds without result, until to-day, when I got Halteridium in a blue-rock-

pigeon (dead three hours). No flagellate forms and no fleas—but I will examine more of these birds. . . .

"Owen's letter is a very funny one. I wish I could send it to you, but he seems to want the matter kept private. However, I think, I may give you the heads of his discourse. He, Dholpur and Haffkine seem to have had a committee meeting over my application. They won't apply for me because they think Government will put me on malaria duty. That is all well, but then Owen goes on to remark 'how fatal it is for any investigator to attract undue attention to his work at a time when the results he has to show are full of doubt and uncertainty.'[1] It would ruin the Health Institute if such an investigator should be brought on its strength and then fail. Then the solution of the malaria problem may lie in another direction to that in which I seem to believe, and (he thinks) I even have an inkling of this! 'You can estimate (he says) what might be your position if such should be the case.' My ideas may possibly change when I commence real laboratory work as they did when you first showed me the malaria parasite. Then he advises me to read the 'vast literature of the subject' and above all to go and see Haffkine, if I can do so 'quietly and without attracting attention,' and he will be able to clear up many 'doubtful points.' I replied thanking him and saying that if the theory were disproved tomorrow I should have nothing to lose but that, on the contrary, the negative result would be important. I might have added that the more attention is attracted to the subject the better, even if the theory prove wrong in the end, especially in a country where every scientific problem is grossly neglected. But what was the use of arguing? This is the common view in this country. If a man is enthusiastic he is advertising himself; if he takes up a subject, he is a faddist and a crank; if he does not belong to some Institute or other, he is a mere amateur! . . .

"Has anyone ever been convinced by argument even to demonstration? Seldom, I believe. Men stick to their prejudices like limpets to rocks—the tighter the more one tries to pull them off.

"What alarms me about Owen's telegram and letter is the

[1] This is an old reproach. Active investigators publish incomplete work partly to help others and partly to get back-details off their mind. Doctors who never investigate think they do it to acquire medical fame—God help us!

fact that he and Haffkine have great influence with the Indian Government and as they don't seem to think much of the theory and hold that it has been too much written up, Government is very likely to be of the same opinion and may *very probably* abandon the malaria investigation entirely on reconsideration. I felt this strongly as soon as I got Owen's telegram; and it was this which decided me to ask you to attempt one last effort. Thin's opinions about the pigmented cells will be sure to be jumped at by all of them, so that I should not wonder if the discovery of those cells, so far from making Government take the matter up, will cause them to drop it *ad infinitum*."

The affair of the Health Institute was amusing. It was the way of the world to suppose that because Haffkine was a distinguished bacteriologist he was also an authority on malaria —which has nothing to do with bacteriology.

In his letter of 17 November Manson mentioned MacCallum's important discovery announced in "last week's Lancet," but did not describe it sufficiently to enable me to understand what it was about; and at Kherwara even *The Lancet* did not exist (page 264).

My next letter to Manson seems to have been that of 13 January 1898. Even then the Tirah campaign was not over and leave was still closed. I acknowledged receipt of my article [29] on the pigmented cells.[1] His remarks on the cells, together with those of Drs. Bland-Sutton, and Thin, and the notes of Dr. John Smyth which I had sent (page 233) were appended to my article. Thin's remarks were very flattering to me, but were otherwise worthless, because he actually imagined that "the bodies in question were the ordinary epithelial cells of the mosquito's stomach containing malarial pigment"—typical example of quasi-scientific medical opinion. I had already controverted this (it scarcely needed discussion) in my letter of 10 November; and now added, in addition: "Dr. Thin has fallen into a horrible pit. His needle-shaped pigment and so on in the epithelial cells are the effect of the formalin! Now I felt that this error ought to be corrected and the two other cases of pigmented cells given.

[1] Though its publication was much delayed I have always been very grateful to Dr. Dawson Williams (Editor, *Br. Med. Journ.*) for taking it at all—most scientific societies and journals would have refused it. My distress at Kherwara was much enhanced by the fear that it would be rejected, that something might happen to me, and that my discovery might be entirely lost to the world.

So I have written a letter to the Editor B.M.J. which they may have room for it." On 19 January some more remarks to the same effect are added, and I said of the cells: "They are the malaria parasite in the mosquito simply because they *can't* be anything else." My second paper on the cells [30] was published on 26 February 1898, and contained an account of my third and fourth positive mosquitoes (page 236) and demolished Dr. Thin's objections.

On 21 January I wrote again:

"I told you that I had found Halteridium in a wild pigeon. Well I shot several more of these birds, together with doves, finches, etc., and found Halteridium in some of the first only. But none of the birds had any kind of ectoparasites—ticks, fleas, etc., though I searched them very carefully with a lens. Then I bethought me of Major Cole's tame pigeons. These are persecuted by a horrible kind of black horse-flies which live among their feathers, flying out when disturbed but always returning even when the pigeon is held in the hand. They suck large quantities of blood from the poor birds; but *wild* pigeons do not have them—so that it appeared scarcely likely *a priori* that the Tabanus (horse-flies) could be the carriers of the Halteridium. But I set to work and examined 30 of the flies. The blood in the stomach when fresh contained numbers of Halteridium, showing that the tame pigeons are also infected; but there were no pigmented cells in the stomach or other tissues of the flies. The histology of Tabanus is just like that of Culex, so that here is another argument against Thin's theory. I have also examined numbers of other Diptera —fleas, dung flies, horse-flies and a few mosquitoes—but no pigmented cells.

"The upshot of all this is that the cells cannot be any kind of physiological cell. . . .

"P.S. Jan. 26th. There are times when I think I may have been sent here as a warning not to preach 'Laveranity' so eagerly. It sounds absurd, but is quite possible. When the District Principal Medical Officer came here the other day he evidently thought something was going on behind the scenes and said it was very odd that I should have been sent here. If this is the case you will probably receive an unsatisfactory answer from Surg. Gen. Cleghorn; and if you do I would

advise the strongest possible action. Fact is I believe my health won't stand many more months of this kind of thing, unless I can manage to get to work here.

"However I hope I may be able to do so. A mosquito bit me yesterday, and it is becoming much warmer. I am paying *touts* to get me malaria cases if possible in the neighbouring villages. . . . I am practising straining mosquito's stomachs. Am also searching for coccidia. Trypanosomes exist in the fish here; but I cannot find hæmogregarines anywhere. If I cannot get to work with quartan, I will try mosquitoes on halteridium."

When applying for the post at Rewa I had hinted to Leslie, Dr. Cleghorn's secretary, that I should probably be forced to retire next year; and now, in his letter of 27 December, Dr. Manson informed me that both he and Sir Joseph Fayrer were writing to Dr. Cleghorn personally urging him to put me on special duty at once, in order to avoid that happening. Cleghorn replied very nicely that he was doing his best, but that I was under the orders of the Madras military medical authorities, who refused to give up their men during the Tirah operation and the plague. I am still of opinion that the discovery of the pigmented cells, if it had been put strongly enough to Government, would have enabled him to obtain my services in spite of these difficulties; and there was nothing whatever to prevent my being sent back to Secunderabad, at least. But the defect of most medical administrators is that they are not strong enough to demand measures which they know are required in the public interest. At the end of January, however, the Tirah campaign was closing, and on the 29th I received the joyful news by telegram:

From Hospitals, India.
To Surgeon Major R. Ross, Kerwara.

Govt. India sanctions your appointment on special duty under director general for six months rupees one thousand per mensem your transfer may be delayed. 27th January.

On 8 February I received orders to proceed. Whatever criticisms of Surgeon-General Cleghorn I had made were now obliterated, and I wrote and telegraphed my warmest thanks to him. We owe some gratitude to his memory.

I announced the news to Manson on 1 February, and added :

" You will be further pleased to hear that I am getting to work again here,[1] though only with brindled mosquitoes. It became suddenly quite warm a few days ago and mosquito grubs appeared in the pots of water which I have arranged for them for the last two months. I obtained some batches of brindled insects on the 29th. I suppose the eggs lie inert during the cold weather. At the same time my touts brought in a number of sick all round. Only two of these contained hæmamœbæ (or coccidia)—a baby with a very few crescents, and a man with splendid triple quartan, one parasite to 150 corpuscles. I thought the brindled mosquitoes might carry quartan. Up to date I have examined 3 fed on crescents and 5 on the quartan. No pigmented cells. I feed on the quartan both before and after the access, for comparison. A number more fed insects awaiting study. It is too cold for them to bite well.

" I am looking everywhere for barred-back and dapple-winged fellows. They are in the station for I find one occasionally in my warm bath-room. I fancy the brindled insects will be a complete failure as regards malaria—as with filariæ. The secret appears to be this. The brindled species breed in *pots* of water round houses ; and the other species in wells and rain-puddles. I shall exhaust the question of the brindled mosquitoes on quartan in a few days, and hope to get on with some other species too.

" 2nd Feb. Three more brindled mosquitoes, one at 4 days and 2 at three days, all fed on the quartan, examined. No pigmented cells. I shall feed a number at 9 p.m. to-night.

" A few grubs have been obtained from an old well. Some of them may be malaria-bearing species. I believe that as soon as I get the proper kind of insects the problem will be solved ; but there is the difficulty. Everything is quite dried up. I shall strain every nerve to get *proof* together before the official enquiry commences.

" You would have been amused at my shifts to persuade the quartan case to let me feed mosquitoes on him."

[1] I give the following extracts here because the Italians afterwards pretended that it was they who had done this important negative work (page 407).

KHERWARA, 8 *February* 1898.

DEAR DR. MANSON,

To continue my report :—I have completed the research on brindled mosquitoes fed on quartan—negative. Since 29th January I have exhaustively examined 34 brindled insects fed on the case of triple quartan either before, or after the access or at 9 p.m. They have been examined from two to six days after feeding. No pigmented cells at all and no signs of pigment being absorbed by the stomach cells or anything like it. I think this proves that brindled mosquitoes are useless as regards quartan and crescents—as they are as regards filariæ. Moreover each of these negative results is a nail in the coffin of Dr. Thin's theory. Stomach cells don't absorb solid particles from the cavity ; and, if they did, there is no reason why they should absorb spent pigment to the exclusion of other particles.

As I suspected, my pots of water all yield brindled insects, whereas three old unused wells have yielded exclusively a scanty crop of grey or barred-back mosquitoes. These while very like brindled mosquitoes differ from them in the shape of the egg which is cartridge-shaped and not oval, and in the larva which has a much longer spiracle and other peculiarities. I have a fair stock of these grey fellows and my quartan subject is still going at the rate of 1 parasite to 1000 corpuscles (no quinine and little fever now owing to systemic toleration) ; but I *cannot* get the creatures to bite. They " simply won't " as *Punch* says of Mr. Balfour. They sit stupidly on the net and won't touch the patient. I have bathed him, put him in the sun to bring his flavour out, and have kept him in the net for hours during the day and also at night. No use. Imagine my exasperation. It is too cold or too dry or something. But I have fed two insects of a closely allied species caught by the hand. One contained no pigmented cells on the 5th day, the other will be examined tomorrow perhaps.

9 p.m. All's well that end's well. Have just received orders to be off to Calcutta at once and report myself to the D.G. thanks to your action. I expected to be kept here for another two months until the war is over. Have just come from trying to feed those grey fellows. Apparently useless ; but have bred a larger variety in earthenware vessels kept by me on the roof of the hospital. Will try them tomorrow.

Now comes the great time. Will prove the theory if I can. Always remember that I consider myself only your instrument in this work. You have directed the telescope: it is my part only to look through it and find the new planet.

9th Feb. No pigmented cells in the 2nd grey mosquito. Will write as soon as I reach Calcutta. Am trying *large* grey mosquitoes. Wish me luck.

Yours very sincerely,

R. Ross.

I hope this proves me to have been a duly grateful disciple.

At that time I still thought that the "swarm-spores" (that is, *Crithidia*) which I had found in the Sigur Ghat and in Secunderabad might be connected with malaria, but I failed to find any here. Some of the larvæ which were obtained from the old well mentioned on 2 February hatched out into *Anopheles* just before I left. Some lectures on malaria which I gave to the native officers were amusing, and as I told my wife: "They stand round and praise me to each other in undertones. 'Isn't he a wonderful doctor sahib,' they say; 'other doctor sahibs could not do this!' They are particularly interested in seeing leucocytes eating malaria germs." But I failed utterly to persuade my hospital assistant to look through a large telescope there was in the station at the total eclipse of the sun which occurred in January. I also went twice to see the Maharawal of Dungapur who was sick—mighty but physically poor potentate—and had one day's mahseer fishing somewhere. Then, saying farewell to my good and kind friends Major and Mrs Cole and their children, I departed from that melancholy exile with a light heart and two bottles full of mosquitoes, on 13 February. I spent another day with the Ravenshaws in Oodeypur, the beautiful city of lakes and palaces, the Venice of India—where, by the way, I was bitten by *Anopheles* (probably *Anopheles rossi*) in the Residency itself and there took train for Calcutta at 11 a.m. Yet another day I spent at Agra in order to see, not only the superb Taj Mahal, but also Mr E. H. Hankin in his laboratory close to it—the discoverer of the mode of purifying wells by permanganate of potassium (page 184), a great benefaction for India; and reached Calcutta on 17 February 1898.

CHAPTER XV

THE PROOF. CALCUTTA, 1898

On arrival I found that I was to investigate not only malaria but also kala-azar, and to do both in six months! Very flattering!—but the whole human race had failed to solve the malaria problem after twenty-four centuries' knowledge of it, and the kala-azar problem is still not quite cleared up. I myself had suggested doing the latter investigation (page 216), but that was before the pigmented cells had been seen; and now I was to find that the kala-azar work was to interfere sadly with the malaria work. Moreover, I was to attack both problems single-handed; not a soul in the four continents but myself was working at the malaria-mosquito theory, and reports on kala-azar had been contradictory. Quite a trifling task! But, nothing daunted, this victim of official optimism cried "Victory or Westminster Abbey," and went at it. The first necessity was of course to recover the lost clue—the pigmented cells. I will allow my letters to tell the story.

8, Harrington Street, Calcutta,
2 *March* 1898.

Dear Dr. Manson,

I missed last mail by accident and so have a great deal to tell you by this one. I arrived on the 17th, after dining with Hankin at Agra on the 15th. On calling on Surg. Gen. Cleghorn, I was told that Cunningham's laboratory has been placed at my disposal; that I am to be on special duty for six months for certain and that if necessary I am to apply for an extension; also that I am to go and work at Kala-azar later on. He asked me what my plans are. I replied that it would be best for me at first to try and get on to the pigmented cells again here and that my further movements should depend on developments. I thought that Kala-azar should be left until the rains, but am writing Giles about it. I think you will agree with this programme. Surg. Gen. Cleghorn has gone [is going]

home and Surg. Gen. Harvey acts for him. I have *carte blanche*, but wish only one thing, that I had been sent back to Secunderabad for two months before the rains begin. However, 'tis a trifle.

Cunningham's laboratory is an isolated building close to a large European hospital, a native hospital and two jails. It has all necessary appliances. For several reasons hospital patients (in other men's hospitals) are not convenient to work with. They expect treatment and the papers might talk. Hence I am starting my touting system with a constant flow of backshish, and have been at work for a week.

Before I left Kherwara I found proteosoma and filaria sanguinis in two sparrows. The large grey mosquitoes would not bite the quartan case, so I brought two bottles of grubs with me. They lived all right. Much to my surprise however I find that the grey mosquitoes are the commonest insects here. All except one out of 19 samples have yielded this species. Brindled mosquitoes are rare and mixed with the grey mosquitoes there are a very few *black or grey dapple-winged insects*.[1] Close to the laboratory there is an old drain swarming with greys and a few dapple-winged fellows. The laboratory itself contains both species in swarms in dusty corners. The greys bite well here but only at night and I hope to feed some dapple-winged to-night. I have to isolate the latter from the former by releasing all together into a mosquito net and then catching the dapple-wings in test tubes, which stopped with cotton are the best homes for mosquitoes. I have fed a number of greys on crescents in order to repeat my former (negative) research on them; but I trust the dapple wings will yield pigmented cells.

There is considerable difficulty in finding cases. I have obtained only four poor crescent cases and I want rich ones. Tertian is also present (2 cases found) and is suitable for greys but one case was a European who wouldn't hear of mosquitoes and the other man ran away. I hunt in the native hospital every morning before 9 a.m. After 10 a.m. I see cases my touts bring me and work till 2 or 3 p.m. At 7 p.m. or 9 p.m. I go back to laboratory to feed my nocturnal insects. There is comparatively very little fever here, the cases being evidently

[1] Probably *Anopheles fuliginosus*.

remittents of a mild type, leading to a few crescents. They say tertian begins with the first rains. Naturally there is at first some difficulty in finding cases but I shall have plenty shortly. I have also bought some sparrows and will examine their blood tomorrow. Will try mosquitoes on them. Crombie has been very kind. Maynard has just returned from leave and will help, I think. Other men are beginning to come to the laboratory. In some quarters there is great scepticism and ignorance not only regarding malaria but other germs.

I have boys out to get me grubs from distant parts and poke about myself after them. Am flooding some ground at the laboratory to imitate a rain-puddle. Weather hot and damp.

If I am not on the pigmented cells again in a week or two my language will be dreadful. Perhaps certain meteorological conditions are required before the flagellæ can take hold in the mosquito's cells.

Directly I arrived here, I wired for my wife and children and they arrived here a few days ago. They will have to go to Darjeeling in a few weeks as it is getting hot here already.

Surg. Gen. Cleghorn was very brusque with me (as they say he always is); . . .

There is so much to be done here that I feel like Aladdin in the cave of the " lamp "—I can't carry away all I can lay hands on. I should like to make a methodical study of all fever cases; also to examine " eye-forms "; also study amœba coli, etc., not to mention animal parasites of animals. I think however I must resolutely set my face against wandering from the pigmented cells at present.

Imagine my disgust on arriving here to find that there is no reference Medical Library. I don't know what to do for some literature I want. I am writing to H. K. Lewis for Councilman and Lafleur on Amœba coli.

3rd March. Examined two dappled-winged insects to-day, between 3rd and 4th day. One had certainly been fed on crescent blood but only a little. The other was doubtful. Neither contained pigmented cells.

Only one out of eight dapple-wings bit the patient last night. I have found a child with a few tertians to-day. Several men are interesting themselves in getting me cases, but getting

under weigh is difficult here. Hope to have good ones by next mail.

With kind regards,

Yours sincerely,

R. Ross.

Have had letters from Dr. Thin, and Thornhill of Ceylon. One of my sparrows has no proteosoma.

Lieut.-Colonel D. D. Cunningham, F.R.S., I.M.S., had for many years carried out official medical investigations for the Indian Government in Calcutta, where he was also Professor of Physiology; but he was now at home and about to retire. His laboratory was a small isolated building containing only two rooms, offices, and a veranda, and was situated in a considerable compound full of trees and bushes [Plate VI]. It was to be my headquarters. There was a native laboratory-assistant and a "durwan," both old men; but I find in my diary for 6 March: "Owing to difficulties in getting any work done by the laboratory assistant and durwan I have entertained from today Mah. Bux and Purboona as assistants at Rs. 8 per mensem," paid by myself, of course. I chose Mahomed Bux out of about twenty applicants because he looked the most rascally of the lot and was therefore likely to have considerable intelligence!—and right well he served me. Later on he knew every bird and every mosquito and gave Indian names to some. Two old hospital beds were placed in the veranda and were furnished with holeless mosquito-nets into which numbers of larva-bred mosquitoes were liberated at night, while the birds in their cages were introduced at the same time. In the morning numbers of mosquitoes were found to have gorged themselves upon the sleeping birds by creeping under their feathers. It was Mahomed's duty to sleep on the ground between the beds in order to keep the cats away—which he did right well. His little fault was that he intoxicated himself with ganja (Indian Hemp) once a week or so. Purboona was the case of crescents on which I had fed the new-dappled-winged mosquitoes; but these experiments were doubtful because Purboona disappeared after he had received his month's pay and Mahomed Bux then told me that he did not remain in the net after my back was turned! Such were some of our minor difficulties.

Surgeon-Major G. M. Giles, I.M.S., had reported on kala-azar, and was soon to write his *Handbook of the Gnats or Mosquitoes* [54], which proved so useful afterwards. Surgeon-

Colonel Crombie, I.M.S., and Surgeon-Major Maynard, I.M.S., were among the first students of the *Plasmodia* in India in 1895 or before. The former was known for his classification of Indian fevers, and was now a District Surgeon in Calcutta; the latter told me in 1895 that he had been "working at the mosquito," and he was now ophthalmic surgeon there; but I do not remember that they subsequently published any work on malaria. I do not remember who the "other men" who came to the laboratory were, but certainly very few if any did so between that date and nearly the end of the year.

I must now explain briefly about "birds' malaria." In 1886, six years after Laveran's discovery of the *Plasmodia* of men [page 119], Danilewsky, in Russia, discovered very similar parasites in birds, and allied ones in lizards and tortoises. But he confused several kinds of these organisms, and later writers were obliged to make minute descriptions and classifications. At this time most people accepted the classification of Alphonse Labbé given in his excellent essay, "Recherches Zoologiques et Biologiques sur les Parasites Endoglobulaires du Sang des Vertèbres" (published in the *Archives Zoologie Experimentale et Générale*, Vol. II, 1894; Paris, C. Reinwald et Cie). He divided the avian parasites of this group into two genera which he called *Halteridium* and *Proteosoma*. Both are parasites of the red corpuscles, contain "pigment," form spores within the blood-vessels, and produce "flagella" when blood containing them is drawn—just as the human malaria parasites do. Since then similar parasites have been found in monkeys, bats, dogs, and squirrels, and the textbook-compilers have indulged in a riot of names for the numerous different species now known. At that time, of course, no one had ever tried to find how these little creatures obtain an entrance into their hosts; and B. Grassi had even refused to consider the mosquito theory because, he said, mosquitoes do not bite birds. Yet the work that I was now commencing might at any time have been more easily attempted in Italy, France, America, and even London. No one seems to have studied the avian parasites in India; and it is a remarkable fact that Cunningham appears never to have observed them in Calcutta, where I found almost every pigeon full of *Halteridium*, though he actually studied pigeons' blood there for other purposes. It is stated that he was good enough to disbelieve in the malaria parasite as he did in Koch's cholera vibrio. Scepticism often saves much trouble.

On page 253 it was mentioned that in his letter of 17

November 1897 Manson had mentioned MacCallum's discovery (page 197, Plate II, figs. 21, 22) to me, but in a manner which did not enable me to understand what it was. But in his of 7 February 1898 he said: "You should go over MacCallum's observations on halteridium and see if it is correct, for, if correct, it is of the very greatest importance. If the polymitus [1] is a fertilising factor in the halteridium cycle then the flagellated body of malaria is also a fertilising factor in the plasmodium cycle. And if this is the case, and the fertilised crescent-derived sphere becomes transmuted into a travelling-cell-piercing vermicule [2] in the stomach of the mosquito we have the explanation of the pigment in your mosquito stomach cells; they are carried into the cell by the vermicule. I assume you have read MacCallum's paper in *The Lancet*. This is a way of reconciling your observations and his; the one set of facts explains the other." Now in Calcutta I was able to obtain the required *Lancet* (of which he had not mentioned the number), and saw at once that his explanation of the pigmented cells was correct—they are the zygotes, the fertilised female crescents which have wandered from the stomach-contents of the mosquito into its stomach-wall and have fixed themselves there (Plate II, figs. 23–30). I had not been able to consider the matter until this second letter reached me, but now saw the full bearing of MacCallum's observation. Of itself it would not have carried us further as regards the mosquito theory. It might have helped to indicate the form of the parasite in the mosquito, but not its position in the mosquito nor the kind of mosquito concerned. Indeed it would have tended to discredit the whole hypothesis by showing that the flagella are not flagellated spores; and it was therefore very fortunate that I had simultaneously found the zygotes growing to a further stage in the mosquito's tissues.

Shortly after arrival in Calcutta I had written to Surgeon Major A. W. Alcock, I.M.S., head of the Indian Museum in Calcutta (now entomologist to the London School of Tropical Medicine), to inquire whether he could help me regarding the identification of mosquitoes and the literature of the subject but he replied as follows on 18 March:

My Dear Ross,

I am sorry to have left you so long unattended to, but I have been busy with exams.

[1] Another name, used by Danilewsky, for the flagellate form.

[2] Another name, also used by Danilewsky, for the fertilised female parasite or zygote.

I cannot discover that anyone has worked out the mosquitoes of this part of the world. I remember, some years ago, that Dr. Ranking made some observations of them but I don't know that he published anything.

I have been looking up our photographic apparatus but cannot find anything for microphotography. . . .

Yours sincerely,

A. Alcock.

I was more fortunate regarding the identification of the birds used by me, which were named by Surgeon-Major Waddell, I.M.S. The crows were *Corvus splendens*, the sparrows were *Passer indica*, and the larks (short-toed) were *Calandretta dukhunensis*. To resume the letters.

8 Harrington Street, Calcutta,
9 *March* 1898.

Dear Dr. Manson,

I have had rather a disappointing week. Six barred-back mosquitoes fed on tertian, and five greenish dappled-winged ones fed on crescents, contained no pigmented cells after from 2 to 5 days. The former experiments are open to question, because the subject, a child with tertian, was in the anæmic stage and possessed on the afternoon before the insects were fed only medium parasites. She was so ill and was otherwise such a bad subject that I sent her away next day. The mosquitoes were fed at night and I am not sure that the child's father did not substitute himself for her. But the five dapple-wings were certainly fed on the crescent case, though only slightly. They are of the family of those in which I first found pigmented cells, but of a greenish colour.

I am having all sorts of petty difficulties at first. Cases can scarcely be obtained though I hunt the hospitals every morning. It is of course the healthy season. The laboratory assistants are useless and I am training men I have hired. Dapple-wings are very scarce and have to be picked out from among thousands of greys, and then bite very badly. All fed mosquitoes die in large numbers for some unknown reason. The truth is, I think that since writing to you the temperature has fallen in a wonderful manner quite ten degrees. I have two blankets at night, which is wonderful for Calcutta in March. It will however be warm again in a few days. Perhaps the

pigmented cells require a certain temperature—or rather the flagella do—before they grow in the mosquito; this mayexplain why they have not been found in the eleven mosquitoes here.

The question of mosquitoes biting birds is certainly settled. Three days ago I put a cage full of sparrows within a mosquito-net full of mosquitoes. Next morning a number of mosquitoes were found to be fed, and on examination the blood in them contained all nucleated [1] corpuscles, but no halteridium or proteosoma. This has been twice successfully repeated. To-night I have six new sparrows some of which I hope have halteridium, I am getting also other birds.

Samples of grubs continue to yield greys, large and small. I think I have made out a sound classification of Indian mosquitoes, based chiefly on the shape of the eggs; namely brindled, with round or oval eggs; grey or barred-back, with cartridge-shaped eggs; dapple-wings, with boat-shaped eggs. There are other notable differences of course.

Many thanks for yours of the 7th Feb., just received. There are many facts in favour of MacCallum's theory. Perhaps you may remember, I thought that the flagella may be of the nature of spermatozoa long ago.[2] Certainly a large number of crescents develop into spheres which do not appear to give rise to flagella. Then you may remember the observation which I mentioned to you last September [4 Aug. 1897] when I saw several flagella attacking one leucocyte. As you say his theory does not affect the mosquito theory. I am thinking it out, and also a method for testing it.

My failure to find pigmented cells in the mosquitoes referred to is annoying as it pushes back indefinitely our prospects of obtaining complete proof shortly. It may however be due to coldness of weather; and I shall employ an incubator shortly if necessary. My great trouble is to get tertian cases, and I have to spend hours every day in the search for them. The greenish dapple-winged mosquitoes, moreover, bite very charily and have to be put to the patient over and over again. Many of them die every day.

[1] The blood corpuscles of birds, fish, and reptiles are nucleated, those of mammals, not.

[2] I cannot find this in my previous letters to Manson, and probably made the suggestion to him before I left England—but must have been overruled. T. R. Lewis thought that trypanosomes might be sperms (*Quart. Jour. Micro. Soc.* 1879).

It *was* unfortunate my being moved from Secunderabad last September. It may be months before I get on the pigmented cells again. . . .

Yours sincerely,

R. Ross.

Congratulations about the parent of the new filaria.

8 Harrington Street, Calcutta,
10 *March* 1898.

Dear Dr. Manson,

On the pigmented cells again to-day (in one mosquito), and have found active vermicules in mosquitoes recently fed on a crow.

Work as follows:—one tertian case found (a European), who failed in getting mosquitoes off himself. Difficulties as to dapple-wings with crescent case remaining. Three more dapple-wings, however, fed on it, and did not contain pigmented cells. Tertian case almost impracticable—quinine.

Seeing these difficulties I set hard to work on birds. Numerous sparrows and larks contained no parasites. Got a new lot; also two pigeons and a crow. Had no time to examine them, so, on the 11th put them all in their cages in a net with a swarm of greys. Next day obtained about 100 fed insects which were put by to incubate; at the same time scanty halteridium was found in crow by puncture and very numerous halteridium of all stages in pigeons. The new sparrows and larks negative. Am consequently working with crow and pigeons. The former has also filariæ.

On 12th also, at noon, fed a few greys who were hungry enough to be awake, on the crow. Blood in stomach examined within an hour. Moving vermicules found at once—about six of them—also some spent pigment. The vermicules were exactly as described by MacCallum, [drawing] moving about sharp end foremost. Other mosquitoes examined up to several hours after feeding showed also a few vermicules whose movement ceased apparently two hours approximately after feeding. Experiments repeated not so successfully next day. Parasites too few for such demonstrations. Obtained about 40 fed mosquitoes off crow and pigeons on night of 12–13 March. They will be examined tomorrow.

Yesterday (14th) I examined a large number of greys fed

on crescent case and birds (night of 11th to 12th). All were negative. I forgot to tell you in my last that a number of greys fed on crescent case have been negative, according with former results in Secunderabad.

To-day (15th) I continued examination of mosquitoes fed on all the birds three days ago [11–12th]. All except one were negative. That one contained two pigmented cells. Of course I don't know which of the birds it bit, but fancy it was the crow. The pigmented cells were in the stomach (I glance at all the tissues formally). One was about 15 μ and the other about 9 μ, each containing about 10 granules of pigment [drawings]. They were just like those formerly obtaining except that the large one had a more definite outline than those found in the four days' mosquito fed on crescents. The body of the smaller cell was almost indistinguishable but could be made out by the arrangement of pigment. The mosquito was of course grey or barred-back, of exactly the same lot as has yielded negative results so persistently with crescents, and lastly with the tertian child, but which contained large cells and swarm-spores when fed on the tertian in Secunderabad.

The mere fact that only two pigmented cells were to be found in the whole stomach goes against their being physiological.

I think I am doing something wrong with my mosquitoes because 80% of them die between 2nd and 3rd day, though I keep them in a large jar with a saucer at the bottom, dead leaves, etc. . . . Dead insects don't seem to contain filariæ or pigmented cells to account for their death.

16th. Examined 18 (eighteen) mosquitoes fed on crow and two pigeons with halteridium three days ago. None contained pigmented cells. Examined also 5 greys fed on crescents—negative ; four white pigeons, 2 jays, 2 dead blackbirds, 1 fever case—all negative. One of my infected pigeons contains one infected corpuscle in six ; flagellates seen in it, but no vermicules. Am pretty well dead beat. . . .

Labbé is poor as regards vermicules. He thinks that nothing exists which he has not seen. Now I find that Kruse, Danilewsky, MacCallum and others agree as to vermicules in crows, and Labbé does not appear to have examined crows. Anyway they existed in my crow a day or two ago. . . .

Owing to paucity of cases have asked Surg. Gen. Harvey for permission to travel to more malarious places. He says sanction of Government of India must be obtained! Just fancy!—that will take some weeks.

With kind regards,

Yours truly,

R. Ross.

CALCUTTA, 21 *March* 1898.

DEAR DR. MANSON,

My one wish is that you were here to share with me the pleasure which I have experienced yesterday and to-day in seeing your induction being verified step by step. Such pleasure comes to but few men, I fancy, though you must have felt it in regard to filaria. I am producing pigmented cells *ad libitum* by feeding grey mosquitoes on larks infected with proteosoma. This, of course, means the solution of the malaria problem.

You remember that on the 12th I obtained a number of greys fed on a crow, two pigeons and several larks and sparrows. Out of nine of these I found pigmented cells in one only. Then I fed mosquitoes on the crow and pigeons and lastly on the crow alone and a few on the pigeons alone. Of these 35 in all were carefully examined—no pigmented cells. Where then did the pigmented cell fellow come from in the first series? Probably from the larks and sparrows. Hitherto I had time only to examine a few of these, in which I found nothing; but now, on the 17th I went through them more carefully, and found *proteosoma*, Labbé, in three larks and one sparrow. What then if the pigmented cell mosquito had come from one of these? It was easy to find out. On the night of the 17th to 18th March 10 grey or barred-back mosquitoes were fed on the three larks. Excitement now rose to a high pitch. On the morning of the 20th I judged these mosquitoes to be ready. All except one were alive. I was so excited that at 8 a.m. I could hardly dissect the first one.

It contained pigmented cells. I now had to come home for breakfast and could scarcely sit it through. When I returned at 10 a.m. I set to work on the rest of the mosquitoes. The second and third of them contained no pigmented cells, but certain peculiar pigment *clumps*. Of the remaining six four

contained more or less numerous pigmented cells—that is, pigmented cells were present in five out of the nine [1] greys fed on proteosoma, and the rest all contained pigment clumps. This, compared with the past negative results, could be interpreted in only one way. I felt that the theory was proved.

Since the night of the 17th to 18th I have been feeding greys on the larks and the sparrows regularly. The insects have been obtained from different sources. Three out of four of those fed on 18th to 19th, and two out of two of those fed 19th to 20th have had pigmented cells. Control insects out of the same bottles fed on crescents have been *quite* negative, both as to cells and pigment clumps. Pigment clumps have been found in *all* the mosquitoes fed on proteosoma, in *none* of the others. What are they ?—we shall see very soon.

The cells are very like those derived from malaria. I will describe them exactly later. Those obtained from mosquitoes after about 38 hours are much smaller than those derived from insects after about 60 hours. To-day I found a different kind of cell in a mosquito which may have bitten the sparrow. I also found *two swarm-spores* in two of the insects. Let us see what I get tomorrow !

23rd March. I have not yet arrived at sporulation but have reached a growth of 40 μ after 85 hours. I find that I exist [2] constantly in three out of four mosquitoes fed on proteosoma and that I increase regularly in size from about 4 μ after about 30 hours to about 40 μ after about 85 hours. I find myself in large numbers in about 50% of the mosquitoes and also in small numbers in about one in two mosquitoes fed on 2 crows with blood parasites. I can't get mosquitoes to bite the pigeons in large numbers.

After about 48 hours many of the cells are found to be quite hyaline, that is, free from vacuoles. After 72 hours almost all the cells look like spherical, almost clear bubbles and are easily seen by low powers. The pigment appears to get less and less the older the cells are, until it is entirely absent sometimes after 72 hours. In most of the cells the pigment is contained in or upon a small internal sphere.

To-day I watched a curious phenomenon. A specimen after

[1] One apparently not dissected.
[2] I am so excited that I turn myself metaphorically into a pigmented cell !

72 hours contained numerous hyaline cells; in 20 minutes they had all become vacuolated. I was beginning to think that the hyaline cells were dead and degenerate ones; or that there were two kinds, hyaline and vacuolated cells. I now find that the latter are the former *post-mortem*, at least with the older cells.

Of course I have only to find sporulation to complete the life history. Then I shall wire to you. I must also feed greys on healthy larks for a formality, and much miscellaneous work must follow. The difficulty as regards sporulation is to keep the insects alive. Now, they all die after 80 hours with the exception of one or two, though the test tubes are changed daily; but last night I fed three over again—the rest refused to bite. I am starting staining to get at the nucleus but haven't succeeded yet. Most of the insects are being mounted in formaline, as before, in series to send to you and to Laveran. I forgot to tell you that I wrote to him and had a very nice reply a few days ago. He wants specimens of pigmented cells.

Though I am working up to 8 p.m., I haven't had time to elaborate the best methods of mounting, etc., as yet. That must follow the discovery of sporulation. I want to keep my specimens this week (to look over them again) and will despatch by next mail. I shall also send crowds of mosquitoes in glycerine.

I am endeavouring to work *backward to* the earliest forms of the parasite in the mosquito. This presents difficulties before 30 hours, owing to blood in the stomach. All the questions regarding flagella, vermicules, etc., ought to unroll themselves now with ease. Before this the difficulty lay in finding the proper species of mosquito, because *a priori*, the flagella or vermicules, may be expected to die in the stomach cavity in the wrong kind.

What an ass I have been not to follow your advice before and work with birds.[1] Technique *much* easier. Was put off by finding nothing in Bangalore.

Illustrations, microphotographs, will be begun shortly. Healthy birds must be infected. 24th. One mosquito lived

[1] I also had suggested to Manson in 1890 that he should work with birds in London; but such ideas were really too obvious to mention. I do not know why I said the technique is easier with birds: is it? I was obviously much excited when I wrote this letter.

to 102 hours and was examined just now (7 a.m.). Cells not much if at all larger but showing tendency to burst. Pigment absent from most of them. Many appearing to protrude from the outer wall of the stomach into the cœlom. I think I saw signs of commencing sporulation in one or two—little oat-shaped bodies [drawing].

That beastly formaline has ruined many of my specimens. Hope to have time to work with other preservatives to-day. . . .

Yours very sincerely,

RONALD ROSS.

UNITED SERVICE CLUB, CALCUTTA,
30 *March* 1898.

DEAR DR. MANSON,

I have not wired to you only because I have not yet found sporulation; but all doubts as to the fact that we have at last succeeded in cultivating the gymnosporidia in the mosquito have vanished. I have been too fagged to write to you daily; but here is the summary of my week's work.

1. The cells continue to be found in three out of four grey mosquitoes fed on larks and sparrows with proteosoma, and on crows with blood parasites (? proteosoma).

2. The cells begin to be noted certainly after one day (between 24 and 48 hours after feeding) as small bodies from 4 to 10 μ or so, profusely pigmented, lying in the stomach wall, sometimes in the external muscular coat, and apparently not intracellular. They increase steadily in size. On about the fourth day they generally lose all traces of pigment, reaching at that time a size of about 30 to 40 μ. They then begin to protrude from the external wall of the stomach into the body cavity. On the sixth day this protrusion is generally complete, the stomach having this appearance: [drawing, like Plate II, figs. 25–29].

At this stage the bodies may reach nearly 60 μ when uncompressed by a cover glass; they are full of a clear fluid containing a few large vacuoles and numerous bright refractive granules, or rather globules, 1 μ in diameter; and their capsule is so delicate that it easily bursts before the cover glass is imposed—which can be prevented by adding a little formalin to the water. The contents are poured out slowly *outside* the

stomach. The cells, though distinct from the stomach wall, still remain attached to it by some invisible tie.

At this stage I thought sporulation would commence; on the contrary it seems to me that the cells now burst or disappear; because in mosquitoes kept after 6 or 7 days (I have examined them up to nearly 10 days, 240 hours after feeding) the largest sized cells are rarely to be found, while a few wrinkled empty capsules may often be seen still attached to the stomach wall. I have not yet determined whether these capsules are natural, or are artifacts caused by the making of the specimen. It seems likely then that some trick of nature takes place at this point. The cells, either bodily or their contents only, should be found in the surrounding tissues—cellular tissues or perhaps attached to the egg. I have not found any certain sign of them yet owing to difficulties of distinguishing normal cells; but have my eye on some suspicious fellows.

3. The size and appearance of the cells therefore corresponds exactly with the *time after feeding*; and it has been invariably found that medium and full-sized cells *never* occur a short time after feeding, or at any time previous to the natural time for their appearance. Hence it follows *that the growth of the cells begins at the moment of feeding and at no* time previously. Also young cells are not to be found in mosquitoes kept for 3 or 4 days without a second feed, which shows that the growth of the cells does not begin after feeding.

4. In mosquitoes which are fed a second and a third time on *infected birds, each feed is followed by a new crop of pigmented cells*—so that we may have two and even three generations proceeding in their growth side by side in the same mosquito. This proves conclusively that the origin of the cells depends upon the feed of blood.

5. No pigmented cells were found in 5 brindled mosquitoes fed on infected sparrows, though they existed in 1 out of 2 greys fed on the same birds at the same time.

6. I think that the cells derived from sparrow's proteosoma differ from those from crows and larks, having a thicker capsule and more scattered pigment.

7. Staining shows (*a*) that the cells take methylene blue, eosin, and logwood equally. With all, when young, I find that each cell contains a number of deeper stained bodies like

bullets in a bag [drawing—like Plate II, fig. 29] separate from a vacuole (?) containing or surrounded by the pigment; but as these bodies take eosine I hesitate to consider them as consisting of nucleinin. Attempts at staining only tentative as yet, of course, and the older cells are especially difficult to deal with. A capsule is well brought out by eosine and glycerine. Meth. blue causes a deep polar staining at one pole in some cases, and occasionally this masses itself into something very like a nucleus. *Certainly* the substance of a young cell contains (*a*) a capsule, (*b*) vacuoles, (*c*) pigment, (*d*) the bullet-like bodies.

8. I found no parasites in three sparrows the other day (one examination with a tired eye). Pigmented cells were found in 6 out of 18 greys fed on them. Hence I believe that one or more of the sparrows contain proteosoma. I have not had time to examine them again yet, but will do so tomorrow.

Conclusions for the week:—

(*a*) Negative results must be obtained from healthy birds; and (*b*) there is a check where the cells leave the stomach which must be got over. There are reasons which make me think that the cells at this point give rise to a large amœba or something of the sort which may sporulate in external nature. It is possible that this check may cause considerable delay.

I think the cells are coccidia in the mosquito; but we must wait before deciding (page 423).

I have prepared a number of specimens for you and wished to look them over before despatching; but excitement and work fagged me so much that my eyes wouldn't hold out today and I had to go home and go to sleep all the afternoon. Feel better now, but my right eye is smarting. Anyway I will send you numbers of specimens and also mosquitoes in glycerine next week.

There is a hand to hand fight with nature; she always has some nasty trick to evade me just as I am getting hold of her. But we will have it all out in a short time.

31st. The *trick* appears to be simply that I have been bursting the coccidia in making the specimen.[1] This morning I looked at a mosquito at the 11th day. The stomach was studded with huge coccidia, over 60 μ without coverglass.

[1] I was not using strong enough salt solution.

While I looked, though formalin was added, they all burst, leaving only shrivelled capsules. Unfortunately my stock of old mosquitoes is giving out.

All the above measurements have been accurately taken with ocular and stage micrometers.

I must now hurry back to work. Feel better again to-day after a good sleep.

Over two hundred mosquitoes were fed last night. This morning [31 March] I found a fourth genus of mosquito with the most singular eggs [*Panoplites*, page 325]. Shall send you insects in glycerine next week.

Yours sincerely,

R. Ross.

CALCUTTA, 5 *April* 1898.

DEAR DR. MANSON,

I have not found sporulation only, I think, because I have not continued to look for it. On finishing my stock of twelve-day-olds I thought I had been trying to rush it too much and went back a little, taking up a stock at 5 days. This showed me clearly that the coccidia reach full size of 60 μ and are quite extruded from the stomach on the sixth day after feeding. [Compare Plate II, fig. 32 and figure on page 286].

. . .

Well having got so far I harked back, as I wanted to occupy some important posts in my rear, which I had not yet attended to. Result—formal proof of a very important question.

You remember that I had found coccidia in 5 out of 13 insects fed on three sparrows in whose blood I had found nothing after an imperfect examination. On the 2nd I examine the sparrows again. One contained nothing in a whole specimen; the second had a *few* proteosoma; and the third *had very many*—so many that I could not possibly have overlooked them, and the bird must have been recently infected (?) This explained the coccidia and gave me a chance to make a fine experiment. I put each bird in a separate mosquito net, and released on them a number of mosquitoes caught in the *same drain* and hatched out in the *same bottle*. You see the test was a perfect one as to the relation between the proteosoma and the coccidia. The insects were examined yesterday and to-day,

and I have mounted 10 from each sparrow for you, beside putting up 8 whole insects from each bird in glycerine for yo to make sections—(they will be sent by *next* mail).

Results were :—

Out of 15 mosquitoes fed on the healthy sparrow an examined I have not yet succeeded in finding *one singl* coccidium.

Out of 19 fed on the sparrow with a few proteosoma an examined, *every insect* without exception entertained coccidia and some contained fairly large numbers.

Out of 20 insects fed on the sparrow with numerous protec soma *everyone* contained coccidia and some contained swarm of them—as many as 40, 30 and so on in *one oil-immersio field !*

Thus at one stroke we get the law :—

" The number of coccidia in the mosquito is proportional t the number of parasites in the blood of the birds."

The experiment is being repeated to-night. The difficult is of course to get a perfectly healthy bird—though perfectio in this respect is unnecessary so long as the law of proportio holds. . . .

What a beautiful discovery this is ! I can venture to prais it because it belongs to you, not to me. I sometimes thin it is the prettiest thing in the whole range of pathology. Ho simple it all is after all !

I think that something funny occurs on the seventh day hence the complete life history may not be found all at onc Of course its discovery implies the solution of the malari problem. . . .

I shall not wire to you until I find the complete life histor I think however that you can safely announce the busine (if you like) when you get this letter and the specimen Perhaps however, better wait for my next letter ; though does not require sporulation to prove that the pigmented cel are cultures of proteosoma in the mosquito.

I wish I could get a good book on coccidia—say *Labbé s les Coccidies des Oiseaux* (Comptes Rendus de l'Académie d Sciences, June 1893). . . .

I am of course forbidden to publish on the subject un I have reported to Government, but I think some announc

ment should be made shortly. I will bet MacCallum will try mosquitoes in a month or two.

Yours very sincerely,

RONALD ROSS.

My letter of 12 April describes a number of specimens sent therewith, especially three series of mosquitoes fed on the three sparrows, with many, few, and no proteosoma respectively. Each series consisted of the stomachs of the mosquitoes, all of which were bred in the same bottle from larvæ caught in the same drain, and all were fed on the respective birds at the same time, and were mounted in 10% formalin. I warn Manson that the pigment in the cells may become enlarged and that my counting was imperfect because I had no movable stage; and I ask him to forward the three series to Laveran, 29 Rue du Montparnasse, Paris. I also send whole infected mosquitoes immersed in glycerine, and added:

"Well, the theory is proved. Not a shadow of a doubt remains that we have found the alternative form of the gymnosporidia. The mosquito theory is a fact. The rest can be done by degrees, and I want to enlarge the scope of operations. By some mistake I am not allowed to travel, and they are still thinking over my request to be allowed to do so. So to-day I *wired* to be allowed to go to the Terai. I shall take my birds, my apparatus, and my servant Mahomed Bux, whom I have trained to the work, with me to the low hills below Darjeeling where I have ascertained there is both accommodation and fever (in the vicinity). After looking round I shall take 10 days leave to Darjeeling where my wife is, and get some much needed rest and exercise. The next bout will find sporulation, and other things, I hope.

"I think you will agree it is time to knock off for a bit, especially as I must get fresh birds and can't hope to tackle sporulation for another week or more. I am dead beat with excitement and work. Shan't let myself work like this again at a temperature of 90° to 100° F. . . . I see Thin had a knock at us at the Medical Society, when he talked about Roger's paper avoiding 'speculative considerations.' The Johns Hopkins Hospital *Bulletin*, Dec. 1897, page 266, also, talks about investigation guided by the 'divining rod' of

preconceived idea—with very evident application to the mosquito theory.

"I think you may safely disturb the serenity of these matter-of-fact people by announcing the cultivation of proteosoma in the mosquito. Maynard will have an editorial notice of it in the May number of the *I.M.G.* I am to report to Government on 17 May, until when I can publish nothing. . . . This will stop the talk about preconceived ideas. Whatever the final development of the coccidia may be, I consider the mosquito theory to be absolutely proved; and pray take this as a deliberate statement of my opinion after three years' study of the question, during which, as you know, I have kept my mind open to the opposite conclusions."

Probably it would now have been an easy task to complete the life-history of *Proteosoma* at once, but three good reasons impelled me to interrupt the work for a few weeks. First I was instructed to send my chief, the Director-General, an interim report giving the proof of the theory which I had obtained, lest my special duty should be terminated at the end of the six months for which it was originally ordered; secondly, as there was little human malaria in Calcutta, I wished to examine the conditions at the foot of the Himalayas before the rains commenced, on the lines of my work on the Sigur Ghat (and I was burning to work with the human *Plasmodia*); thirdly, the wretched kala-azar problem worried me, and there were similar diseases at the foot of the Darjeeling hills; and, lastly, I was suffering from overwork and the great heat. When my family arrived in Calcutta in February we lived at a boarding-house at 8 Harrington Street; but on 24 March they had gone to Darjeeling (Ada Villa Hotel), and I stayed first at the United Service Club at Calcutta and then with Dr. Maynard close to my laboratory. On 14 April, after considerable delay, Government gave me leave to travel to the Terai (the plain at the foot of hills); and I therefore applied for ten days' leave to Darjeeling, in order to write my report and determined to make my headquarters afterwards at Kurseong, a little station on the mountain railway between the plains and Darjeeling, from which I could descend to various parts of the Terai at will. It was hoped that I could there, not only continue the work on avian malaria, but also resume that on human malaria, which had been so unfortunately interrupted in September 1897. Away we went therefore in

Elliott & Fry.

RONALD ROSS, 16 NOVEMBER, 1899.

LABORATORY AT CALCUTTA. SURGEON-MAJOR ROSS, MRS. ROSS, MAHOMED BUX, A LABORATORY ASSISTANT, AND BIRDS IN CAGES, 1898.

a body—Lutchman, Mahomed Bux, and numbers of cages full of crows, sparrows, pigeons, and larks—to the utter amazement of the other railway passengers, European and native, who thought I was a lunatic to keep such pets.

PANKARBARI, DARJEELING DISTRICT,
3 *May* 1898.

DEAR DR. MANSON,

I am rather afraid I have missed this mail; but will chance it.

I left Calcutta on 17 March (April). A murrain occurred among my birds the previous night, carrying off all my proteosoma sparrows and a young crow with some kind of gymnosporidium, but I brought the rest with me. I left my man, Mahomed Bux, at Kurseong, 5000 ft. up with orders to ind mosquitoes and have a stock up to a week or more by the end of my leave. I then went on to Darjeeling, 8000 feet (train all the way) and took my ten days' holiday—that is I worked only about 4 or 5 hours a day. With me I took a large stock of mosquitoes fed on proteosoma sparrows and another large stock fed on crows with halteridium. All the former had coccidia and none of the latter had. The former stock I carried to 9 days. Results as before. The coccidia reached 60 on the 6th or 7th day and then no *marked* further progress (see further down). Having done this, I selected a good series for Laveran and sent them to him with a long letter and careful drawings announcing the establishment of the mosquito theory. I did not tell him about the three series A, B, & C which I sent to you, as I thought you might not like to part with them, but I suggested he might write to you on the subject. I also sent you a number of greys fed on crows with halteridium and killed early next morning for sectioning for vermicules. Lastly I made a number of drawings to scale for my Report, as carefully as I could—the photos turned out failures. This was my holiday task. On the 28th I returned to Kurseong where I found my man with a stock up to 6 days—no coccidia. He had also hunted for mosquitoes—a variety of grey (as also at Darjeeling). They bit well at Kurseong. I remained there two days trying to get sparrows—failed. Examined several little birds—negative. Examined 10 pigeons lately from the terai (foot of the hills);

all had halteridium badly. Some had tabanus and lice—no coccidia in these. Couldn't do much up there, so, on 1st May, I came down here.

This place is 2000 feet above sea and a few hundred above the terai. Seems to be no fever here, but plenty below. Find considerable difficulty about getting birds (which I want to carry out life-history of coccidia) but hope to have some tomorrow. Shot one miner (bird not man) [Maina]—full of some species—free forms. Mosquitoes mostly grey here just as at Kurseong and Darjeeling. Planters are getting me fever cases. There is a small hospital of four beds here. Found two anchylostomiasis cases straight off and am starting cultivating the ova according to Giles. Also a horrible case of what they call cancrum oris here. The pus was full of a huge fungus (? ray fungus), also swarms of flagellate organisms (cercomonas ?), also (?) *coccidia.*[1] Will examine tomorrow again.

I fancy discoveries are to be made here for the picking up —but must stick to my last.

Now to return to the proteosoma-coccidia of the 6th–7th day. My progress is greatly hampered by want of new literature on coccidia. Leuckart and Blanchard are old and I can't get Labbé's paper on coccidia of birds. What are microsporidia among coccidia ? I fancy the proteosoma-coccidia are going to turn out to be of this nature and that the spores will form only some days after the insect dies in the water. . . .

Yours sincerely,

R. Ross.

The letter concludes with descriptions and drawings of full-grown "coccidia"—the latter being published in [32] and [33].

PANKARBARI, 9 *May* 1898.

DEAR DR. MANSON,

Yours of the 21st April just received. I know you are anxious for greys killed shortly after feeding on proteosoma, but will you believe it, here I have been for 10 days, and can't get a single bird with that parasite. I am nearly wild. Sparrows all round and can't catch them. A few young ones and 2 old ones were free of it. The local idiots called Butias

[1] I think that these bodies in ulcers had been previously mentioned by D. D. Cunningham, and that they were possibly *Leishmania.*

can't do it; I and my man have practically failed; and now I have set the police and the district magistrate on it. Fancy the whole local Government trying to catch sparrows and failing! Out of 19 larks sent me from Calcutta 17 arrived dead this morning! It is simply maddening. No use shooting them of course. A planter has however made me a new trap, and the head of the police says that if he can catch decoits he can catch sparrows. We shall see. . . . You say that if only you could find a vermicule entering a cell—"Eureka"; but I claim full proof already on the strength of the differential experiments plus the general structure and growth of the coccidia. I think you will admit it after seeing the specimens.

There will be no difficulty in tracing the vermicule business by ordinary dissection and washing out of the stomach [1]; but hitherto I have kept my mosquitoes to the second day for fear of spoiling my averages by missing the parasites so early on the first day. I have now done enough in the differential line and will go in for development on the first day and after the 7th day. Thanks for your tip about dissecting in salt water. I will use it. [I had often used it already, but too weak]. . . .

. . . I am making a careful study of Terai sickness hitherto considered to be "deadly malaria." Out of half a dozen cases only one contains a few wretched small amœboid bodies. All have swarms of ankylostome eggs besides ascaris and trichocephalus, also amœba coli and cercomonas, in many. Coin me a word like *herodiasis* and you will express exactly the resulting condition. It is not one parasite but many; the wretches are being literally "devoured of worms."

Now I have no doubt that a similar condition holds in kala-azar but it is a difficult matter to say which of all these parasites is the predominating one. So far as I can see the question can only be answered after a careful preliminary examination of the chronic malaria and anchylostomiasis separately. But this will take ages.

In the meantime, if you see Surgeon Gen. Cleghorn and he is recovered sufficiently from his accident to talk about the matter, I think I would ask your discouraging my rushing

[1] Owing to the third interruption I never had time to do this easy but interesting little piece of work.

off to Assam for a long time yet. The malaria problem is large enough in all conscience and is much more important than the kala-azar question. I can't do two things at once. A stock of mosquitoes is quite enough work for one day; and so is a stock of patients each with half a dozen parasites, temperature to be taken (native hospital assistants are not to be trusted even to do this), diet arrangements to be made, stools to be kept separate, and so on. Both will not go into one day. Let malaria be the first thing and let me do odd jobs on the rest during odd corners of time as at present. Besides I won't be ready for kala-azar without much more careful clinical work on the terai sickness. Remember the mass of work before me in regard to finding the species of mosquito or other suctorial second hosts for each of the gymnosporidia separately. . . .

Have you done any more about pigment in lymphocytes?

KURSEONG, 16 *May* 1898.

DEAR DR. MANSON,

I am in a vein of bad luck and haven't been able to obtain a single subject. No malaria cases. The birds which at last I was able to catch (about 20) round Punkarbari had no proteosoma and consignments of sparrows and larks from Calcutta and elsewhere arrived dead. But I have just got 15 sparrows from Calcutta and will examine tomorrow. I am consequently working all day at the Report which Surg. Gen. Harvey told me to submit after three months. It will be called "Report on the Cultivation of Proteosoma, Labbé, in Grey Mosquitoes," will give full details and be copiously illustrated with drawings to scale. I claim full proof of "Manson's Mosquito Theory" and solution of problem of cultivation of the gymnosporidia and hope you and Government will be satisfied with my first three months' work. On looking through my notes (which I keep very carefully, entering every observation) I find that the result of the differential experiments is absolutely conclusive.

Out of 245 grey mosquitoes fed on birds with proteosoma 178 or 72% had pigmented cells.

Out of 41 do. fed on crescents, 154 on halteridium, 25 on a healthy sparrow and 24 on birds with immature proteosoma or a total of 244, *not one contained a single pigmented cell.*

There is no fudge about this, and only one error—tha

of an insect which had been left in the net after feeding on proteosoma.

The presence of pigment and the general development of the proteosoma-coccidia, as I name them provisionally, completes an absolute proof. Hence I am justified in reporting (apart from orders) at this stage, especially as sporulation will properly belong to the second part of the enquiry. . . .

Your letter of 9 April is full of business matters. You say that after the position is taken, the matter should be handed over to assistants, and you think you could work it through the Royal Society. Well, the position *is* taken, and I think that the sooner that the affair is put on a more extended footing the better. I would suggest that, as soon as you feel satisfied of the correctness of my recent work, vigorous action be taken at once to do so. My reasons are (*a*) my special duty lasts only until 17 August; (*b*) I may be taken ill and have to go home at any moment; (*c*) they want me to tackle kala-azar; (*d*) the work is too much for one man; (*e*) the opportunity is a grand one (and should be seized without delay) for starting some *commission* or *deputation* with some degree of permanency, to work the general subject of *animal parasitism in India*.

I urge immediate action. . . .

Here is the programme for a Research Deputation for Indian Animal Parasitism.

Malaria Carriers to be found for halteridium and each of the three species of human malaria—mosquitoes to be classified —their habits considered—their prevalence in different localities determined—an immense work. The sporulation of the coccidia, development in water if it exists, mode of infection of birds and men all to be determined and a 100 other questions.

Amœba coli, cercomonas and perhaps coccidium oviforme in men (vide Giles' Report on kala-azar which I trust)—immense subjects.

Worms, especially ascaris and anchylostoma, of intestines, and filaria. To differentiate between multiple infections of many parasites is also an immense task, and I believe half the sickness usually attributed to malarial cachexia is really multiple parasitism—herodism, to coin a word.

Ulcers—an unworked field almost. Parasites of animals.

Mark this, *our present system of medicine in India is antiquated, vicious and ignorant* (as you have so often implied); it will be the duty of the Deputation to reform it, or attempt to do so. Our present diagnostic methods are those of the middle ages; what we call malaria, dysentery, hepatitis, malarial cachexia, and so on may be anything. Our methods of treatment are based on the treatment of the symptoms not of the diseases. We give patients tonics, diaphoretics, quinine when we should give thymol, and astringents and dieting when the intestine remains infested with parasites. Not only our hospital assistants and our assistant surgeons, but our very medical men are absolutely ignorant of the elements of tropical disease. The hospital assistant at Punkarbari had never *even heard* of anchylostomiasis, and a young medical practitioner here, who is working in a malaria and anchylostoma country, told me himself that he had never seen the malaria parasite, did not know an anchylostome egg by sight, and was quite ignorant of the worm and its effects.

If Chamberlain takes up your ideas the Government of India and the Royal Society will.

Don't ask for any *building* for a laboratory. We don't want laboratories, we want men. . . .

Yours sincerely,

R. Ross.

KURSEONG, 30 *May* 1898.

DEAR DR. MANSON,

My work here has been brought to a full stop by the *plague scare* of which you have doubtless heard. The people, incited by wire pullers and encouraged by the extreme weakness of Government think or rather pretend they think, that Government wants to have them forcibly inoculated. Every medical man becomes an object of suspicion, and you can understand I am especially so. The coolies from two tea gardens near Punkarbari bolted in a body and the local magistrate and one of the planters have begged me not to work again just yet, and don't like my living anywhere except at Kurseong here, at the hotel. At the same time owing to the rain it is too cold here now for mosquitoes to bite, so though I have at last got some sparrows from Calcutta with pro-

teosoma, I haven't been able to feed a single insect. My man Mahomed Bux, is looked upon with worse suspicion than myself and in fact started the scare by shooting at sparrows near Punkarbari [page 280]. They said there is a doctor sahib shooting coolies preparatory to inoculation! I have therefore had to wire to Simla for permission to return to Calcutta; but am going to-day to the house of a planter about 2,500 feet to endeavour to feed mosquitoes. . . .

I have carefully thought out future work and have come to the conclusion that it is *impossible* for me to attempt kala-azar yet unless I drop malaria altogether. It will only mean a similar waste of time to that which has occurred during the last six weeks. *One problem at a time.* I think then I should make every endeavour to complete the life-cycle of proteosoma, and work at hæmamœba as a secondary item. But kala-azar and anchylostomiasis should be dropped entirely. I think you will agree with me; *if so, will you write to that effect to Surg. Gen. Cleghorn.* I have already written to Surg. Gen. Harvey and have begged him to let me off kala-azar for the present. I don't know that he will without your backing.

As urged in my last letter I would also suggest my being put on animal parasite research on a more permanent basis as soon as possible. In my letter to Surg. Gen. Harvey I asked for an extension of my present duty and I hear that *Cunningham retires on the 26th June.* . . . Set me down specifically to solve the great problems in animal parasitism—malaria, amœba coli, anchylostomiasis, etc., etc., then I will do good work.

If I am hampered right and left, I won't do good work; and I think I should add that as my pension is due in three weeks I feel pretty independent. I think you will agree that back bone is necessary; my pension is my backbone; and I haven't forgotten Kherwara yet. Don't want to be undisciplined at all; but also don't want to be a jelly-fish. . . .

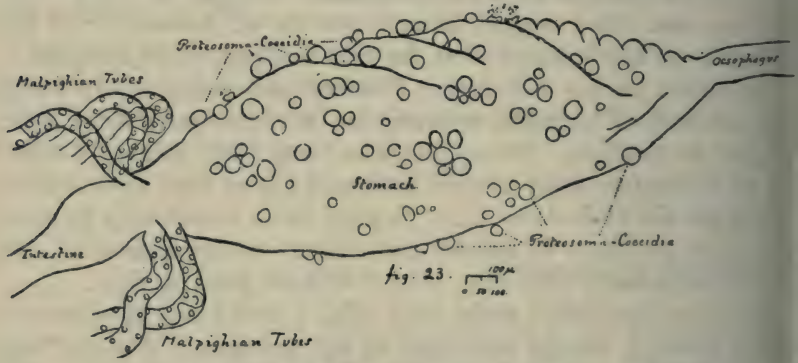
Yes, it was certainly the "pigment dung" in the coccidia which led me on to them; but I think I should have found them out without it because they are such obvious bodies on the 4th and 5th days. The whole difficulty has been, not to find the mosquito phase of the parasite, but to find the mosquito. In short it is a conquest not of observation, but of faith. Faith carried us through all the years of negative

attempts with brindled and grey mosquitoes. The fact that flagellation occurs *after* the blood is drawn was the finger that pointed always in one direction only; but the road was such a rough one that it required faith's boots to get along it. I doubt whether mere *observation* of vermicules, etc., would ever have done it. The vermicule found, the next thing would have been to find what became of it. Mosquitoes would have been possibly tried, and ten to one the wrong species. Result negative. The road closes here, they would have cried. But we looked up and saw the finger of *theory* still pointing the same way. . . .

Yours sincerely,

R. Ross.

My Report to the Director-General, entitled "Cultivation of Proteosoma, Labbé, in Grey Mosquitoes" [32], was finished on and dated 21 May 1898, and was sent in from Punkarbari, Darjeeling District, on 24 May, with a covering letter in which



STOMACH OF MOSQUITO WITH FULLY GROWN PARASITES.

I claimed that "full proof has at last been obtained, after three years' investigation, of the mosquito theory of malaria," and begged earnestly either for assistance or for release from the kala-azar duty, if only because (among other reasons) the "severe strain on the eyesight will render it a physical impossibility to continue at the same time the equally laborious malaria research." With the unconscious cruelty of stupidity or indifference, both requests were rejected.[1] My Report contained nineteen foolscap pages of print, with notes of thirty

[1] Surgeon-General Harvey afterwards apologised to me for this (page 390).

three different series of experiments made since 1895, and results, descriptions, discussions, technique, and nine large plates drawn by myself. The receipt of it was acknowledged by Surgeon-Major Leslie ; and it was to be printed at once, but not published without the consent of the Secretary of State in London ! I subjoin one of the figures.

I communicated all the facts also to Laveran in my third letter to him dated 25 April, and sent him a number of specimens.

While my Report was being printed Manson, at my request, published an abstract of the work in a paper called " Surgeon-Major Ronald Ross's Recent Investigations on the Mosquito Malaria Theory," 18 June [33], in which he gave the salient facts, with three blocks of drawings.[1] He described the development of the " proteosoma coccidia," said that he wished " to place on record Ross's claims to priority in discovery," and added Laveran's statement, made to him in a letter, that, " The discovery of Dr. Ross appears to me, as to you, to be of very great importance. . . . I have shown the preparations to M. Metchnikoff, who shares my opinion."

By a most singular coincidence, the wide publication of these strong opinions by three of the most distinguished authorities in France and England seems to have exactly coincided with the moment when that ingenious " zoologo," Professor B. Grassi, of Rome, first sallied forth into the wilderness with the fixed determination to prove the whole mosquito theory of malaria *indipendentemente da Ross*. How well he succeeded in this is one of the curiosities of the quasi-scientific literature of academies and textbooks, and the matter will be dealt with in Chapters XVIII and XXI.

For myself I hold it to be a point of personal honour to acknowledge in the warmest terms possible the help of other workers ; and I therefore close my letter to Laveran of 25 April with the words :

" Will you permit me to conclude with an expression of satisfaction that after three years' labour I am at last able to make this announcement to you, who, not only originated our correct knowledge of these subjects, but from the first divined that the mosquito is connected with the propagation of these parasites."

[1] The paper contains one error, probably due to some ambiguity in one of my hurried letters. It suggests that I had found the development of *Halteridium* as well as that of *Proteosoma*.

And at the end of my " Report on the Cultivation of Proteosoma " I wrote :

" These observations prove the mosquito theory of malaria as expounded by Dr. Patrick Manson ; and, in conclusion, I should add that I have constantly received the benefit of his advice during the enquiry. His brilliant induction so accurately indicated the true line of research that it has been my part merely to follow its direction."

Several men of science, even in India, afterwards reproached me for this last sentence, saying that it was not true and that Manson himself should at least have attempted to realise his own speculations ; one even said to me : " Let Manson praise Ross, but not Ross, Manson." The fact was that at that time he had many enemies who were attacking him virulently in the medical Press for having unjustifiably claimed, they said, to be physician to a certain hospital. I had not followed the controversy, but attributed the attacks merely to the monkey-vice of jealousy, and was very indignant. I was and am a hero-worshipper, and he and Laveran were my heroes. He, almost alone, had helped me in the long fight, and I swore to give him all the credit I may have acquired in return. So ! Let the cup of gratitude spill over rather than be stinted. It is true that his induction indicated only some suctorial insect—mosquito, flea, tick, anything ; that he was wrong about the flagellated spores and that I should never have arrived further than MacCallum's discovery if I had only " followed the flagellum," as he kept on adjuring me to do. But his induction did something more ; it led me to pursue the parasites from man into the suctorial insect, not *vice versa*, as the guesses of King and Bignami [9 and 21] suggested ; and it kept me from wandering away into the barren wilderness of epidemiology, as I almost did in 1897 and as B. Grassi did now. That is why I said that it indicated the true line of research. So it did.

From this point, as will be seen, Manson's prognostications all failed. Laughing Nature showed us a miracle more wonderful than he had ever dared to imagine.

My Report was in print shortly after Manson's article appeared, and I sent copies to him, Laveran, Thin, John Smyth, and many others from Calcutta before the end of June.

The " plague-scare " alluded to above was really a scare

not against plague, but against Haffkine's excellent anti-cholera and anti-plague vaccines. These had been in use (owing to Haffkine's ability and energy) for some years; but the usual cranks and agitators now saw their opportunity and began to persuade the ignorant populace that the Government were inoculating them, not against plague but with plague. Numerous riots occurred and the Indian Government, which was even then beginning to show signs of weakness, seemed unable to discriminate between liberty of speech and liberty of lying, allowed the agitators full scope for their falsehoods, paralysed sanitary efforts, and thus probably permitted thousands of people to die of preventible diseases. Of course, if any doctor pricked a man's finger in order to obtain a drop of his blood for examination, he was thought to be inoculating plague; and, even before I left Calcutta, the matter had become so serious that the military Principal Medical Officer there advised me not to touch the patients in the native military hospitals, which, with the scarcity of cases at that season, further checked my work on human malaria. When I went to the hills I found the scare established there also, and when Mahomed Bux shot a sparrow for me in a village, all the coolies, some hundreds in number, on the neighbouring estate of Longview ran away in a body (page 284), costing the planters, my friends Mr. J. D. Gwilt and Mr. G. C. Dudgeon, some thousands of rupees! I wonder they ever forgave us. But they were always most kind; and Mr. Dudgeon, who was an excellent amateur entomologist, gave the drawings and description of my "grey mosquitoes" (*Culex fatigans*) which I published in my report, and which I could not get from the Indian Museum.

My pleasure at returning to those great mountains, which I now saw again for the first time after the years of my early childhood, may be imagined. My work lay mostly in the gigantic trench, somewhat like the Sigur Ghat, which leads up northward from the plains to Darjeeling, richly wooded, with a torrent at the bottom of it, and broken foothills on either side. Punkarbari lay low down near the west flank of it—a rest-house, planters' hospital, and planters' bungalows. The railway ran up the east side of the trench, and Kurseong was perched near the top of the steep declivity looking out at the plains to the south and, from a point on the road, to the eternal snows of Kinchinjunga to the north. But at that season the heights were haunted by immense clouds; and even during the ten days I spent at Darjeeling I saw the mighty

mountain but once—suddenly, after a day's rain, breaking out, like the phantom of another world lit by the setting sun, above the rolling and grumbling legions of mist—just as my father before me painted it.

Those days were for me days of victory—spent among the magnificent mountains which are themselves symbols of victory. They stand gazing down for ever upon the plains of common earth—

> The farmyard grey with its sheaves of garner'd grain,
> The village and all its far-off cries, and the herds
> With tinkling bells,

themselves—

> Black bastions of the cloud-conquering crags,

and victors over Time and Fate. So, I thought, we poor creatures who must toil below may sometimes by our own efforts haply reach some higher summit; and during that brief holiday I planned and commenced the humble verses of mine which I love best, with a new rhythm and a rich harmony of words, my song of triumph, *The Indian Shepherds* [117] —really the postscript to *In Exile*, but never finished (page 509).

The Muse of Exile is adjured to rise from her sorrowful meditations, to ascend the mountains, and to hear the songs of the shepherds there.

> Hear, mournful Muse; mute Mistress of Exile, hear;
> Sad Spirit of Solitude and of these barren briars,
> Who dwellest with tigers and serpents in this waste
> And waterless wilderness. . . . Wake! Arise!
> Disconsolate daughter of a perisht past. . . .
> . . . Why dreamest thou thus weary
> Of drawing for ever at the deep wells of thought
> Unnourishing wisdom; weary of man and of all
> His worthless works; and not of thyself the less:
> And weary of wisdom and her lies; of love
> Her hate; of law her crime; of virtue her vice;
> And of justice her injustice: knowing not good
> If it be good, or evil if evil; o'erwrought,
> The painful reason ravelling her own clue? . . .
> Make haste and arise. Ascend to those uttermost peaks,
> And hear the glad songs of the Shepherds there and rejoice.

And the poem proceeds with:

> The Song of Shepherds on those Himalayan heights,
> Enormous peaks of enormous mountains that face,
> Far over all India and her silvery streams,
> Sirius and Canopus, the South and the sounding sea,
> In range over range.

CHAPTER XVI

THE MODE OF INFECTION. CALCUTTA, 1898

When I returned to Calcutta I knew that it was for the last stage of the great battle—the decisive victory. In April the proteosoma-coccidia (zygotes) had been traced up to their growth after seven days. This seemed to be their maximum growth, for afterwards they would burst at a touch on the cover-glass. They were evidently ripe; yet when they burst nothing very visible came forth. I dissected mosquitoes in plain tap water or in "normal saline" (a weak, 0·4 solution of salt), and preferred the former because it made the *young* zygotes more visible. Manson had recently suggested the latter, thinking that I had not been employing it; and this suggestion gave me another idea: that a *much stronger* salt solution should reveal the contents of the *ripe* zygotes.

Calcutta (care of Postmaster, Calcutta),
13*th June* 1898.

Dear Dr. Manson,

As anticipated in my last I was obliged to return to this place owing to difficulties in getting birds with proteosoma and in persuading mosquitoes to bite at Kurseong and Punkarbari. I arrived here on the 4th. Owing to plague scare all the bird catchers had bolted, and my sparrows from Kurseong died on returning (to their own climate!), so that I had some difficulties in getting under weigh again. At last however I have some beautiful stock of proteosoma-bearing greys and shall be hard at sporulation in three more days. The plan I shall adopt will be to examine only dead insects, as, all considered, I think the spores will form some time after death. At the same time I have a number of healthy young birds which I will try to infect by cramming malariated mosquitoes down their throats.[1] I have just ascertained beyond

[1] These experiments were to verify Manson's conjectures that the *Plasmodia* would now enter the water in which mosquitoes containing them had died, or would be swallowed as "mosquito-dust."

a doubt that birds eat mosquitoes. Out of 70 which I tried to refeed last night only about 50 were found in the net (a new one) this morning.

The difficulties are minor ones—the birds are killed by too many mosquitoes, the insects die in numbers at each refeed and when they lay their eggs. There is also difficulty in getting numerous sparrows here at present. But it is quite possible I may complete the life cycle of proteosoma in a few weeks.

I am also getting dapple-wings [*Anopheles rossi*] in large numbers now in *rain-water puddles.* These I am going to try on *halteridium.* Of course I am not allowed to experiment on human beings here because of the scare—at least just yet. . . .

Surgeon General Harvey has ordered 100 copies of my Report; which is being run through the press as soon as possible. The plates are to be photolithographed. I hope it will arrive in ample time for the July meeting [of the British Medical Association]. He has however refused to *publish* the report, though he says I may send copies to experts. . . .

14th June. Nearly 30 of my best stock died last night. It is a beautiful stock, every mosquito having numerous coccidia. Also my best sparrows gave up the ghost. Owing to the heat, I expect, the coccidia in the dead mosquitoes are unusually large for the period—4th day reaching 45 μ, some of them and protruding freely. Hence I have left 20 of the dead insects to rot in water with a view to seeing what becomes of the coccidia. Sporulation *may* take place before complete growth, as in birds' proteosoma. . . .

I fear I have lost time in going to the Terai but hardly expected the sparrows there to have no proteosoma, nor to be hindered by the plague scare. I might have finished proteosoma here and then come home for the Tropical Section. Anyway it is possible I may be home in September or so, if I can infect birds.

I applied for assistance. Refused of course. Things will never be done on a proper scale until a fuss has been made. . .

Yours sincerely,

R. Ross.

CALCUTTA, 22 *June* 1898.

DEAR DR. MANSON,

. . . I think I am on the verge of another grea

advance. I have kept dead mosquitoes until they have fallen to bits—no development of the coccidia of 4th to 7th days. They resist longer than the stomach tissues, but evidently degenerate—capsule shrinks, contents contract and become yellow, and ultimately bacteria enter. In the last stage of decomposition the stomach consists of a bag, the outer coat, full of bacteria, and having empty capsules of coccidia attached to it on the outside [see Manson's speculation, page 129].

Hence no opening here apparently. Next the former suspicion is being confirmed that the coccidia burst in the *living insect* on about the 7th or 8th day, because in insects after that date (when fed only for the *first time* on proteosoma-blood) only empty capsules seem to occur [Plate II, figs. 33, 34].

But I have found a most useful reagent—fairly strong salt solution. This brings out in a wonderful manner the *striation* of the coccidia and makes them look like caterpillars contained in an egg. Is it karyokinesis ?

No, I think it likely that the striations are due to the coccidia, or some of them, being packed full of *germinal threads* 10 μ in length which are *scattered into the living insect's cœlom.* I say this on the experience of one insect only of 8th day examined this morning. It contained many mature proteosoma occidia deeply striated ; and also swarms of small bodies [drawing—see figure, page 295] *feebly moving* in the salt solution and evidently escaped from the body cavity of the insect. I thought that by pressure I could force similar bodies out of the coccidia. The bodies are not bacteria ; they taper at both ends and are broader in the centre, which appears capable of some change of shape.

23rd. Have just found the " germinal rods " again in a mosquito of 8th day whose stomach was covered with empty capsules. Think I am on the track and hope to give you definite news by next mail.

I have heard from Laveran ; the specimens I sent him have not arrived though I registered the parcel. . . .

Yours sincerely,

R. Ross.

It had been arranged that a new Section for Tropical Medicine was to be created at future annual meetings of the British Medical Association. The first of these was to be held at

Edinburgh near the end of July, and Manson was to be the first President of the Section. I was straining every nerve to send him specimens and to complete the life-cycle before that date—as in fact I did.

The first part of my letter to Manson of 28 June 1898 is missing from the bundles of originals, but three of the missing paragraphs were found by my secretary, Miss Yates, amongst Lord Lister's correspondence when she was engaged in copying my letters to him (page 375). I suppose that Manson had sent him the sheet. The letter proceeds :

Another advance has been made. On the 6th and 7th day the coccidia form a large number of germinal rods (?) 12 μ in length. This is easily seen by using a rather strong salt solution, which gives many of the coccidia from 5th day a striated appearance as figured in the report, but more marked. By using pressure these coccidia are easily burst, when the rods pour out.

They are just about 12 μ in length and all the same size. They taper toward each end and are therefore thicker in the middle. In salt solution after a time, they show one or more vacuoles in the middle part and become bent angularly. They stain easily with Loeffler, Delafield, etc. and then show one or more chromatin granules toward the middle, between the vacuoles, and the same time become broader, when they look very like small *trypanosomes*. They often possess swellings somewhere along their length, very like flagella do. On addition of water, they tend to curl up and look like granular material. I am not sure whether they are at all motile, though I have often thought so. I think especially that they bend and straighten themselves, but these points are difficult to make out because of their continual Brownian movement, while it is [?] that both water and salt solutions kill them.

When youngish striated coccidia are burst these bodies are found attached like rays to circular cells [Plate II, fig. 31], and others similar.

This arrangement can often be seen within the unburst coccidium. The circular cells to which the germinal rods are attached stain easily, like the rods, but shew no nucleus.

In older coccidia the rods pour out all separately and the cells are not to be found.

The rods are easily seen in large numbers in eight day

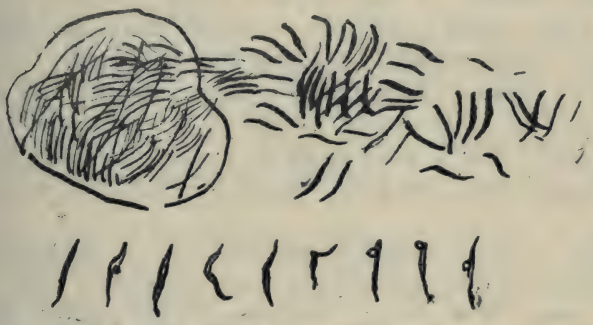
mosquitoes (and earlier) all round the stomach and amongst the abdominal scales. They are in the cœlom.

The use of fairly strong salt solution has enabled me to ascertain these points, which may be taken as quite certain.

No development of the rods has yet been observed either in living or in decomposing mosquitoes.

One never finds all the coccidia even when mature, to be striated, that is, to contain rods. The rest seem to contain nothing but a clear fluid with bright oil globules.

I have searched in vain in living mosquitoes and dead ones for anything like psorosperms or pseudo-navicellæ, unless



"GERMINAL RODS" POURING OUT OF A MATURE "COCCIDIUM." FROM LETTER OF

ERRATA

Page 294 : delete second paragraph and replace with the following :

CALCUTTA, 28 *June* 1898.

DEAR DR. MANSON,

I despatched some copies of my report to you by this mail with one for Sir J. Fayrer. Will you oblige me greatly by sending it on to him. I don't know his address, but will write to him as well.

I fear much I shan't be able to send you good second-day specimens as my infected birds are so much better that they don't yield good crops any longer ; but will try to-morrow.

The bulk of the copies of my report are not sewn even yet and will never reach the Secretary of State through the Government of India in time for the B.M.A. meeting. The only thing to be done then is for you to send him one of the copies I have despatched to you with Leslie's letter, and ask his permission to publish. He is sure to give it straight off.

Page 294, line 32 : for "[?]" read "evident" and insert diagram.
Page 294, line 34 : for "are found" read "issue".
Page 294, line 35 : after end of paragraph insert diagram.

order to complete the life-cycle. What the dickens she is going to do next I cannot imagine at all. Great difficulties may however be feared owing to the absence of hard spores. But I am trying to turn the position by blind attempts to infect healthy sparrows.

Infected mosquitoes have been fed on five healthy sparrows for a fortnight. On examining again the other day a few proteosoma were found in one of the sparrows—scarcely good enough. I am therefore feeding three healthy sparrows with mosquitoes containing mature coccidia. [Still trying to verify Manson's conjectures.]

But this climate! I can perhaps stand another month of it. It is hell, simply; and the constant strain on mind and eye at this temperature is making me thoroughly ill. If I cannot infect birds or find a clear opening by the end of July I *must* clear out. Permanent work here is out of the question for me, I feel. But I shall stick on to the last gasp in order to be able to wire to you before the B.M.A. meeting, if possible. . . .

29 June. I have found large numbers of germinal rods in the thorax of two mosquitoes, one a *six* day and one an *eight* day one. The former contained two empty capsules and two striated (rod-bearing) coccidia; the latter contained only a large number of empty capsules. The rods lay among the scales and muscles of the thorax and showed no further development.

This scarcely looks like the spermatozoa theory. The rods appear much too numerous for this and sometimes *all* the coccidia in a mosquito are rod containing ones. They must evidently get into the insect's circulating fluid for some reason. . . .

Thin's advice to work for years and years is good. It is just what I have done. It is also good not to publish until there is not a vestige of objection possible. Here again the Report follows his advice.

My advice is that any further objections are scarcely to be recommended as they will meet with a terrible fate, I think. I don't see any line of criticism which can be taken up against the theory with any chance of success. I venture to say that there is no fact in the life history of any parasite more certain

than that proteosoma in the bird becomes coccidium [1] in the mosquito. It is quite certain. The man who criticises will die; he will knock his head against a rock.

Many thanks for your efforts on behalf of a commission. Remember always two things that the Government of India is a *mule* as regards science. It won't do anything unless driven. As for the I.M.S., it does not care a cent for the whole business. Remember also that if the Government of India won't do anything I am always willing (if not anxious) to take my pension and work elsewhere and for other masters. What do you think of Egypt? . . .

CALCUTTA, 6 *July* 1898.

DEAR DR. MANSON,

Let me in the first place felicitate you and the profession on the new Tropical Section and let me wish it every success in the future. It is started under the best possible auspices.

I hope this letter will reach you at Edinborough [sic]. If so, it will be opportune; because I feel *almost* justified in saying that I have completed the life-cycle, or rather perhaps one life-cycle, of proteosoma, and therefore in all probability of the malaria parasite. I say almost, because though I think I have seized the final position, I have not yet occupied it with my full forces.

My last letter left me face to face with the astonishing fact that the germinal rods were to be found in the thorax as well as in the abdomen. Instead of the hard resisting spores we expected to arrive at—spores easily seen and followed—here were a multitude of delicate little threads, scarcely more visible than dead flagella and poured out amongst the million objects which, under an oil-immersion lens, go to make up a mosquito. I dare say you imagined my consternation. I could not conceive what was to happen to the rods.

Well, I was in for a battle. It was, I think, the last stand—on the very breathless heights of science. I am nearly blind and dead with exhaustion!!—but triumphant. Expect one of the most wonderful things.

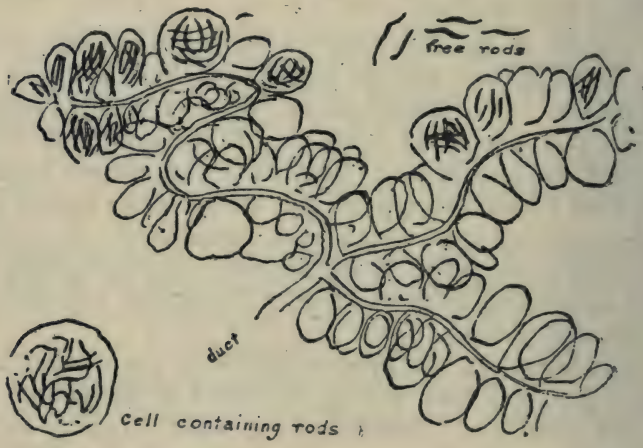
The rods were evidently in the insect's *blood*. By merely pricking the back of the thorax and letting the milky juice flow into a minute drop of salt solution thousands of proteo-

[1] Of course the term *coccidium* here is merely provisional. I meant coccidium-like bodies. Obviously I had no time to consider mere names (page 424).

soma-coccidium rods could easily be obtained. The question was what next ?

I now divided my insects before dissection between the thorax and abdomen and examined each part separately. It was found that the rods were often *more numerous in the thorax than in the abdomen* ; there were even cases where scarcely one could be found in the abdomen (the coccidia having evidently burst some days previously as shown by their empty capsules), while numbers (4 or 5 in a field) could be detected on tearing up the thorax and head.

Here however I was brought up standing. Sometimes the



GERMINAL RODS IN SALIVARY GLAND OF MOSQUITO. PUBLISHED, FROM ROSS'S DRAWING, BY MANSON IN "BRITISH MEDICAL JOURNAL," 24 SEPTEMBER 1898, PAGE 852 [34].

rods were more common in the head, sometimes in the thorax. I went at mosquito after mosquito spending hours over . . .

[Unfortunately a part of the letter is here missing—probably sent to Lord Lister ; but the missing part certainly described the following observation, recorded in my notes for 4 July, for Mosquito No. 22 : " On taking up the thorax, which was first separated from the abdomen, a small round clear mass of tissue fell out and was seen to consist of mostly some worm-like gland with an air tube down the middle. Rods were very numerous here and were seen to be enclosed in numbers in clear

cells attached to this gland (diagram)." The missing part of the letter probably contained the drawing on the opposite page.]

mosquitoes. In six of them the cells were packed (especially in some lobes) more or less with germinal rods. In the seventh I could find only a small piece of gland, which was free from rods.

ERRATA

Page 298, line 13 : delete dots, also whole of last paragraph and first two lines on page 299, and replace with the following :

" each, until I was blind and half-silly with fatigue. The object was to find if possible a place or structure where the rods accumulate ; or to discover some further development in them. Nothing.

On the 4th, however, after pulling out the head by its roots (œsophagus, etc.) from the thorax, some delicate structure dropped out of the cervical aperture of the latter. This proved to be a long branching gland of some sort, looking like a coil of large intestine, and consisting of a long duct with closely packed refractive cells attached to it. I noticed at once that the rods were swarming here and were even *pouring out* from somewhere in streams. Suddenly, to my amazement, it was seen that many of the cells of this gland *contained the germinal rods of proteosoma-coccidia within them.* Looking further, the cells of one whole lobe of the gland were simply packed with them, and on bursting the cells the rods poured out of them just as they pour out of the original coccidia.

(Diagram here)

The rods were quite unmistakable, having the tapering, flattened, and vacuolated structure peculiar to them. They are identified at once and no structures like them exist in the normal mosquito. Here they were in the cells of the gland. The cells were not coccidia. They are only 25 microns in diameter, have a very thin outline and contain a perfectly clear fluid, without granulations or oil-drops such as the coccidia possess. The rods lay within them quite irregularly and motionless except for Brownian movement. In one lobe almost every cell contained numbers of rods ; in other lobes only one or two cells contained them. By the attachment of the cells to the central duct, it seemed quite easy for the rods to pass on occasion from the former into the latter.

Now what was this gland ? Will you believe it, I examined two whole mosquitoes without finding it again ? What with the scales, the debris of muscles, etc., I could not come upon it. A third mosquito gave the same result, until I opened the *head* itself. There was the gland *attached by its duct which led straight into the structures somewhere between the eyes.* The cells were again packed with germinal rods. I have found the gland now altogether in seven."

soma-coccidium rods could easily be obtained. The question was what next ?

I now divided my insects before dissection between the thorax and abdomen and examined each part separately. It was found that the rods were often *more numerous in the thorax than in the abdomen* ; there were even cases where scarcely one could be found in the abdomen (the coccidia having evi-

cells attached to this gland (diagram)." The missing part of the letter probably contained the drawing on the opposite page.]

mosquitoes. In six of them the cells were packed (especially in some lobes) more or less with germinal rods. In the seventh I could find only a small piece of gland, which was free from rods.

I still experience, however, the greatest difficulty in dissecting out the gland itself. It appears to lie in front of the thorax close to the head, but breaks so easily in the dissection that I cannot locate it properly. In the second mosquito however there was no doubt, as shown by evident attachment, that the duct led straight into the head-piece, probably into the mouth.

In other words it is a thousand to one, it is a *salivary gland* [Plate II, fig. 35].

I think that this, after further elaboration, will close at least one cycle of proteosoma, and I feel that I am *almost* entitled to lay down the law by direct observation and tracking the parasite step by step—

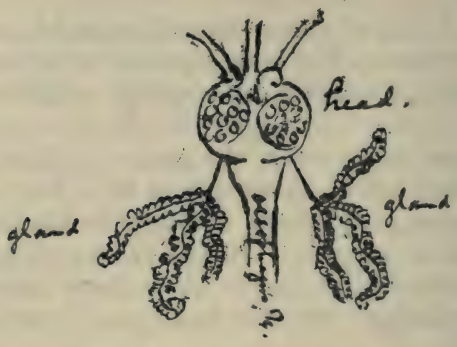
Malaria is conveyed from a diseased person or bird to a healthy one by the proper species of mosquito, and is inoculated by its bite.

Remember however that there is virtue in the " almost." I don't announce the law yet. Even when the microscope has done its utmost, healthy birds must be infected with all due precautions.

I say *one* cycle. I think it likely there is another. I continually observe that only a portion of the coccidia contain germinal rods. The rest, I now think, give rise to the black sausage-shaped bodies shown in Plate I, fig. 20 of my Report [Plate II, fig. 34], which I believe may be the true spores of the parasite meant either for free life or to infect grubs. Oddly enough, in old mosquitoes these bodies also get carried away into the tissues—unless they are some disease of the insect. I will attack this next.

7th. I dissected two healthy mosquitoes this morning and began by dividing the head and anterior third of the thorax from the middle third by means of a razor, and then carefully breaking up the anterior third. In both cases the glands

were found and their ducts were traced straight into the head ; thus :—



In all probability it is these glands which secrete the stinging fluid which the mosquito injects into the bite. The germinal rods, lying, as they do, in the secreting cells of the gland, pass into the duct when these cells begin to perform their function, and are thus poured out in vast numbers under the skin of the man or bird. Arrived there, numbers of them are probably swept away by the circulation of the blood, in which they immediately begin to develop into malaria parasites, thus completing the cycle.

No time to write more.

Yours very sincerely,

R. Ross.

CALCUTTA, 9 *July* 1898.

DEAR DR. MANSON,

One full solution of the malaria problem was obtained this morning—unfortunately just too late to catch the mail by wire to Bombay. The facts are as follows :—

Just after I wrote to you I obtained some good dissections of the gland in healthy insects. It still remains very difficult to get out nicely and almost impossible to mount for you ; but by examining a number of insects I have obtained the leading facts about it. The lobes lie either in the head, neck or front of thorax (according to the fulness of the cells, I think) I think there are 18 lobes :—thus [drawing—there are only six, as I soon found].

The ducts finally all join together into one large tube which

runs up along the under surface of the head. In one specimen which I will send you I can trace it to its termination. When it arrives close to the origin of the mouth parts, it (the duct) passes through a kind of round shield and enters one of the long stabbing spears of the proboscis. It appears to run up *inside* this and open somewhere near its end.

There is no doubt then that the gland must pour its secretion straight into the wound made by the mosquito.

In old infected mosquitoes I think that many lobes of the gland are almost destroyed by infection with the proteosoma rods. In one case the cells seem to have united into a single sack loaded with rods.

In 8 male mosquitoes I have not once found any evidence of the existence of this gland.

So far then, there is a complete chain of evidence pointing to the infection during haustellation. Now for the clincher.

On the 25th June (as I think I told you) I carefully selected three healthy sparrows. Their blood had been examined three times on different occasions and always found free from parasites. On the 25th night and almost every night following I have used these birds to refeed a large stock of mosquitoes which had been infected from diseased sparrows on 21st–22nd June. This was the identical stock with which I worked out the story of the germinal rods. Lately I had been finding young crops of coccidia in this stock and therefore had a presentiment of what I should observe in the birds. I wish therefore I had examined them before so as to let you know in time for the B.M.A. Meeting; but I hardly expected such a knock-down result.

All three birds, perfectly healthy a fortnight ago, were now simply swarming with proteosoma, twenty parasites and more in one oil-immersion field.

Again on the 1st July I selected a healthy sparrow and a healthy "baia," and refed another infected stock on them every night up to date. These two were examined this morning. Both had proteosoma, the sparrow a few, the baia fairly numerous.

Hence I think I may now say Q.E.D., and congratulate you on the mosquito theory indeed. The door is unlocked, and I am walking in and collecting the treasures.

For instance, proteosoma of sparrows is communicable to baias. Hence there is probably only one proteosoma, common to various birds. You see I shall give it to crows, etc. Also, I have an idea indeed. What if proteosoma is spring tertian? I shall feed infected mosquitoes on a man or two, and shall take it from a man to a sparrow, if it be so.[1]

11th July. I have forgotten to say that 10 (ten) healthy sparrows examined on the 25th June and kept nightly in mosquito nets to keep mosquitoes *out*, all remain perfectly well up to date. I told you also that on 25th June I gave infected mosquitoes to three healthy sparrows by the mouth. Two of the birds died a few days afterwards (numbers of my birds have been dying) without proteosoma. The third had nothing on 1st July and was used for feeding infected sparrows; with positive results as already given. Rather a jumble, but future experiments will be more precise. It's useful to be jumbly at times to begin with.

I have now to record three more successes. Three baias (kind of bunting I think) were formerly found to contain large numbers of halteridium only—no proteosoma I am certain. They have often been used to refeed old infected stock. Yesterday they were examined again and found to contain numerous halteridium, and *swarms* of proteosoma. One bird died last night. Liver enormous and black. Heart's blood contained an enormous young brood of proteosoma [drawing]. The bird's blood practically consisted of parasites. There you have it full at last. This also *proves* that sparrow's proteosoma in communicable to baias.

Having won the battle of Atbara I am now marching on to Omdurman; that is, I am rushing the black-sausage-shaped bodies. . . . [I failed!]

Well, I have become unbearable with conceit over tracking the germinal rods into the salivary gland. That was a grand charge. I brag openly about it! . . .

The scheme you are going to propose is admirable. No doubt the Government of India will jog up when Chamberlain comes along. Remember I am entirely at your disposal. The idea is grand, and I promise to throw myself into it.

[1] Mahomed Bux, who had a bad attack of *P. vivax* later on, thought he acquired it by being bitten by *Proteosoma*-infected mosquitoes.

I have already arranged my tactics. They will be Napoleonic (!) —concentration. We shall attack one problem at a time in a body, if possible. That is the way, I am sure. See the results I am getting out of proteosoma alone. . . .

. . . One single experiment with crescents (there are numerous dappled winged mosquitoes here now) will enable me to bring human malaria into line with proteosoma—they are sure to be just the same. This will really score all the chief points. I ought to do it in two months ; it has taken me only one to work out the germinal rod cycle. . . .

I have had 3½ years incessant work and worry, am feeling tired and out of sorts and feel I should have a rest and get a sniff of cold before opening into such a big business as a commission. . . .

. . . We are nearly ruined from having to live in hotels everywhere—family in one place, myself in another. . . .

Yours sincerely,

R. Ross.

CALCUTTA, 20 *July* 1898.

DEAR DR. MANSON,

. . . Your letter to Harvey with Lord Lister's enclosure ought to do everything as to my being kept on at this work ; but they won't give me assistance without pressure from the India Office. It is an excellent idea to send me a couple of men from the Colonial Office to learn the work. Last week I wrote asking to go home, and I should certainly like to do so ; but if you think it necessary will stay out longer in order to teach your men. *Do just as you like and think fit.*

Five more birds (sparrows) have been infected. Four healthy sparrows were bitten by old mosquitoes with germinal rods on the nights of the 10th–11th, and 11th–12th July. They were examined again on the 18th. One was free (and died of tetanus same evening) the other three now swarmed with parasites, respectively, 2, 6, and 10 in each field—proteosoma only of course.

On the 12th–13th and 13th–14th June, 5 healthy sparrows were bitten by the same old stock (3 weeks old). Two died next day and one on the 19th (no proteosoma). The remaining two had moderate proteosoma on the 18th day, (sixth day of experiment).

Out of 4 control sparrows, one shewed a few proteosoma on the 7th day—probably overlooked before. The rest remain quite healthy.

I have calculated by Calculus of Probabilities the chances against birds becoming infected by accident *within a fortnight* after an experiment, on the basis of the percentage of infected wild birds I have found. The chances are millions to one against 3 or 4 birds showing simultaneous accidental infection within the period. Apart from this, nine of the birds (I have infected 13) have had much more severe infections than I have ever seen before. I can therefore announce the law :—

"Healthy birds may be infected by the bites of the proper species of mosquito which have previously fed on diseased birds."

I shall bring mathematics to bear heavily on these matters presently. Lucky I know something of the science (spent seven years over it). It will serve to knock on the head many loose ideas.

Not much work possible on the black bodies this week as I have been keeping my old stock for infecting sparrows. But I have failed to find the bodies in a large number of healthy mosquitoes, so that I think they must be spores of the coccidia. They are carried by the circulation and distributed all over the tissues. The largest number of them seen by me in one insect is two or three hundred. A specimen kept for three weeks in the damp chamber shows no marked alteration or development of the bodies. They are too few to be meant to infect birds directly by the mouth or lungs. The chances would be infinity to one against one of them ever entering by these channels. What is a dead mosquito in water or dust? It is infinitesimal. Men and birds don't go about eating dead mosquitoes. No, nature is too clever for such an attempt. She brings the mosquito (and the infection) straight to the man or bird and puts it nicely into his blood, so as to give it every chance. I think then that the black bodies must be meant to infect grubs. They will probably produce germinal rods and infect the salivary glands again.

I am thinking of telegraphing to you, giving the result of the infection experiments. It may help you at the B.M.A. . .

Yours very sincerely,

R. Ross.

CALCUTTA, 27 *July* 1898.

DEAR DR. MANSON,

On the 25th, some more birds having become infected particularly a crow, I made up my mind to wire to you announcing that one complete solution of the problem had been obtained. I think it may strengthen your hand at the B.A. Meeting and elsewhere, and only wish I had done it before, as I might well have done.

On the same day yours of the 8th was received. It is dated the day before my first experimental batch of birds were found to be infected so that " following the spore into the bird " had been practically done when you wrote. . . .

Much work has been doing. I forget how many birds have been announced to you as infected. Several fresh birds have come in, in spite of the fact that I have used very few mosquitoes. The only difference caused by doing this is that some of the birds (which were not bitten) escape. Those bitten have all very severe infections, quite exceeding what is ordinarily seen. The parasites begin to appear on the 6th, 7th and 8th days and shortly reach vast numbers, 5, 10 and more in a field when the bird dies. I found five young proteosoma in a single corpuscle.

I told you I would give sparrow's proteosoma to a crow. So I have. Parasites appeared first on the 9th day but have not become very numerous. They are utterly different from crow's halteridium and exactly like sparrow's proteosoma. These details must be left for future work, and I am concentrating on the " black bodies." . . .

Yours sincerely,

R. Ross.

Not only my " Report on the Cultivation of Proteosoma " [32], but also my letters, specimens, and telegram announcing the discovery of the " germinal threads " in the salivary glands of mosquitoes and the experimental infection of healthy birds by the bites of mosquitoes duly reached Manson before the meeting of the new Tropical Diseases' Section of the British Medical Association, which was held at Edinburgh on 26 to the 29 July 1898. There, as President of the Section, he gave a masterly account of this induction and of all my subsequent work up to the infection of birds as far as 25 July, when my telegram was sent [34]. This announcement was made

less than six weeks after he had communicated the cultivation of *Proteosoma* on 18 June [33], and, according to Dr. Charles (page 339), it "created quite a *furore*." My friend, Professor W. J. Simpson, who was present, has given me the following description of the event.

31 York Terrace,
York Gate, Regent's Park,
Nov. 18, 1921.

My Dear Ross,

It was at the Edinburgh meeting of the British Medical Association in July of 1898, that Sir Patrick Manson was Chairman of the Tropical Disease Section. I attended all the meetings of this section and I remember the profound sensation produced among the members when Sir Patrick read out a telegram he had received from you describing your success in conveying Malaria from bird to bird by means of "grey" mosquitoes fed on Malaria infected birds. At the same meeting was shown microscopical specimens which you had sent him demonstrating the progressive development of the Malarial parasite in the mosquito. A resolution was unanimously passed sending you the Members' congratulations on your great and epoch making discovery.

I am glad to hear you are writing your memoirs.

With best wishes, believe me,

Yours sincerely,

W. J. Simpson.

Other distinguished men present were Dr. William Osler of Baltimore and Professor Raphael Blanchard of Paris, the author of *Zoologie Médicale*, which had always been of so much use to me. The latter wrote me from Edinburgh on 28 July asking me for an article on my subject for his new *Archives de Parasitologie*, and adding, "P.S.—M. Manson a fait ce matin à la British medical Association une communication du plus haut intéret sur vos recherches. Il me prie de vous presenter ses meilleurs compliments." The resolution of congratulation was proposed by Dr. Andrew Davidson, of Edinburgh, the author of *Hygiene and Diseases of Warm Climates*, was seconded by Sir Joseph Ewart, and ran:

"That this Section, recognising the great importance of the researches of Surgeon-Major Ross on the development of the proteosoma in the mosquito in its bearings on the ætiology

of human malaria, desire to convey to him through the President its deep sense of the obligations which all students of tropical pathology are under to this distinguished observer, and to assure him of the interest and hope with which it follows his work."

The Section also thanked Dr. Manson "for having placed before the Association the latest discoveries of Surgeon-Major Ross with which his own name is so inseparably connected."

These details are given because someone was good enough to deny later that the meeting ever occurred! A full account of it, together with a copy of Plate I of my *Proteosoma Report*, was published in the first number of the new *Journal of Tropical Medicine*, edited by Dr. James Cantlie (London), which appeared in August 1898; another, with one figure, in *The Lancet* of 20 August; and an even fuller account, with several editorial notes, was given in *The British Medical Journal*, 1898, for the 24 September, together with the same plate and two other figures, taken from the missing parts of my letters copied above, which delineated the "germinal rods," by themselves, and within the cells of the salivary gland. Appended to this last version, there is the following note:

Later Observations

"*Note.*—Letters received from Ross subsequently to the delivery of the foregoing confirm and expand what is therein stated. Many (30) successful experiments on the communication of proteosoma to healthy birds, indicate an incubation period of from five to nine days. The intensity of the infection so conveyed gradually increases during several (4) days, until finally as many as from five to ten or more parasites are to be found in every field of the bird's blood. These artificial infections are much more severe than those acquired naturally; the birds may die from them." [The note goes on to record my observations supporting Mannaberg's view that the sexual forms may be produced by conjugation, and my studies of the black spores.]

Manson was doubtful whether malaria is communicated only by the bite of the mosquito, for he said:

"*Bearing of Ross's Observations on the Malaria of Man*

That this is the last word on the subject of the extra-corporeal phase of the malaria parasite I do not believe.

I think that malaria may be acquired in this way—that is, by the bite of the mosquito; but that that is the only way I cannot venture to assert, in fact I do not think. For observe; malaria, we know, multiplies indefinitely outside the human body, independently of man. In fact, malaria is most prevalent in places where man is not.[1] Therefore, this extra-corporeal condition of multiplication must demand something more than a short cycle of from man to mosquito and from mosquito to man. My impression is that it requires an infection of mosquito by mosquito; possibly, as in tick disease or as in silk-worm disease through the insect's ova, possibly through the larva. Ross's facts supply us, gentlemen, with an explanation of the former sort. They do not tell us how the parasite multiplies indefinitely through generation after generation. Still, it is a most important addition to our knowledge, and I am sure that it will lead to a full solution of the entire problem and to very many advances in this important subject. It may be objected that what holds good for proteosoma may not hold good for plasmodium malariæ; but the similarity of the parasites is so great that one cannot resist the conclusion that their histories are also similar. Moreover, Ross has distinctly shown, as already mentioned, that certain species of mosquito do elaborate pigmented cells when fed on human malarial blood.

"I am sure that you will agree with me that the medical world, I might even say humanity, is extremely indebted to Surgeon-Major Ross for what he has already done, and I am sure you will agree with me that every encouragement and assistance should be given to so hard-working, so intelligent, and so successful an investigator to continue his work."

As shown in my letter to him of 20 July, I was not exactly of this opinion: I thought that nature "brings the mosquito (and the infection) straight to the man or bird and puts it nicely into his blood," and that this was the only way in which *men* and *birds* became infected; but I also thought that the "black spores" possibly infect *mosquito grubs* from their parents, which may have died in the water in which they laid their eggs, thus carrying on the infection from mosquito to mosquito independently of men or birds. But this was only

[1] I showed later, especially in [66], that there has never been any real evidence for this statement. See also my letter to Lord Lister, page 386.

a speculation requiring experimental proof, and in the meantime the fact remained that malaria was carried from man to mosquito and from mosquito to man.

Thus the great victory was won at last. It gave us simultaneously two important advances in knowledge.

Zoologically, the case of *Proteosoma relicta* was the first one in which the great law of Metaxeny or Alternation of Host (page 122) was proved to apply to unicellular parasites as it had been known for years to apply to higher parasites; and it was certain by zoological homology to occur (though possibly with some small modifications) in other members of the same group, including the three species of malaria parasites of men, and also, very probably, in some other unicellular parasites.

From the *epidemiological* point of view the discovery showed us that the malaria infection is not acquired from drinking water, nor from the inhalation of marsh air or "miasmata," but is put directly into our blood by the bite of mosquitoes. King had thought of this in 1883 (page 124); Manson had not supported the idea; I had considered it possible (page 190); but none of us had really even imagined the wonderful process which had now been discovered on 4 and 9 July. Nature was more resourceful and astute than all of us. She did not spill the germs broadcast through the soil, water, and air, as we had supposed; she put them directly into man from the mosquito and into the mosquito from man! And this would render more instant and exact the means of preventing the greatest of diseases.

Apart from such considerations, the entry of the "germinal threads" into the veneno-salivary glands of the mosquito is surely, perhaps, the most wonderful and beautiful case of natural design which we know of—difficult to explain by evolution even to-day.

I will not attempt to describe my feelings to the reader—especially as he may already be weary of such excursions. Then indeed—

> Then felt I like some watcher of the skies
> When a new planet swims into his ken.

Such moments come only to one or two persons in a generation. The pleasure is greater than that given by any triumph of the orator, the statesman, or the conqueror; for the end attained does not lie in some petty intertribal advantage, but in a benefit conferred upon all men, and, not only for to-day, but for all time—at least until "The future dares forget the past."

The triumph of 20 August 1897 was now completed and crowned by that of 9 July 1898—more than enough to compensate me for all my toils.

But now Dame Fortune, who had conferred such a great bounty upon me, tightened her lips and her purse-strings and did nothing but buffet me for the remainder of my time in India! First she sent me the "black spores" to worry me for weeks. Then she gave me the dapple-winged mosquito, *Anopheles rossi*, which does not carry malaria at all. She continued the plague scare. She made the Royal Society refuse to send me help. She instigated Surgeon-General Harvey to interrupt my work for months on the kala-azar inquiry; and lastly, she arranged that some ingenious Italian gentlemen should pirate most of my results in the interval. "Souvent femme varie, Bien fol est qui s'y fie!"

The *black spores* were first discovered by me, appropriately enough, on 1 April—large, brown, sausage-shaped objects. As they occurred in clusters actually within the ripe *Proteosoma-coccidia* and (at first) only in infected mosquitoes, it was natural to infer that they might constitute a developmental stage of the parasites; and as it was my first duty to work out thoroughly the whole life-cycle of *Proteosoma* before beginning more showy work on the human *Plasmodia*, I was obliged to expend much valuable time and labour on the subject. I tried to infect birds and mosquito-larvæ with them—in vain; and it was not until January 1899 that I found them in uninfected mosquitoes and began to be convinced (I had already suspected it, page 295) that they were only parasites within parasites. They did not occur at all in the infected mosquitoes in Sierra Leone (page 386). They are now thought to belong to the genus *Nosema*. One of my quasi-scientific friends later accused me of being "deluded" by these bodies; but I was quite right to investigate them thoroughly.

The *Anopheles rossi* were large dapple-winged mosquitoes, not unlike those in which I had first found the pigmented cells in Secunderabad in 1897 and also like those I saw in Oodeypur. They began to appear in my laboratory about this time, but I was still not allowed to work with human malaria there, owing to the plague-scare. My men, however, found their larvæ—in water on *the ground*.

Some few more extracts from letters to Manson. On the 3 August I wrote:

"The black spores refuse to capitulate; the result is that

I am nearly dead with work again. They *don't* change in water or in dead mosquitoes, though kept—for periods ranging to six weeks nearly. They *don't* change in the stomach of medium and large grubs. By feeding these on torn-up mosquitoes with black spores I can get as many as I like (as many as 500) of the latter into their stomachs—but not a sign of change. I am now trying very young grubs and have also fed five healthy sparrows with the spores.

"Numerous most beautiful demonstrations of the germinal rods (what *shall* we call them ?) in the salivary glands; but fixed specimens almost impossible to make. Also some very good specimens of coccidia of tenth day breaking up into black spores. . . . I am dying to finish proteosoma as I am beginning to knock up more than ever. Nothing but work as hard as I can go for 2 months. Am beginning to be sleepless. (Illegible) fear all the same, as you may imagine but most trying to eyes and brain.

"As you say, proteosoma should be finished and then the human germs can easily be tackled. Unfortunately that kala-azar weighs on me like a nightmare. I do hope you will be all right when this reaches you."

On 11 August I wrote:

"I have not much to tell you this week. Three out of five birds fed with 7 infected mosquitoes each on the 31st—many of the mosquitoes containing black spores—have all remained healthy to date; while several birds bitten on 3rd to 4th night were attacked the day before yesterday. Apparently no outlet here. I have fed many grubs of all sizes on the spores. Crowds of the spores in their stomach tube, but they are passed out and found in the droppings quite unchanged. Spores in the damp chamber remain the same.

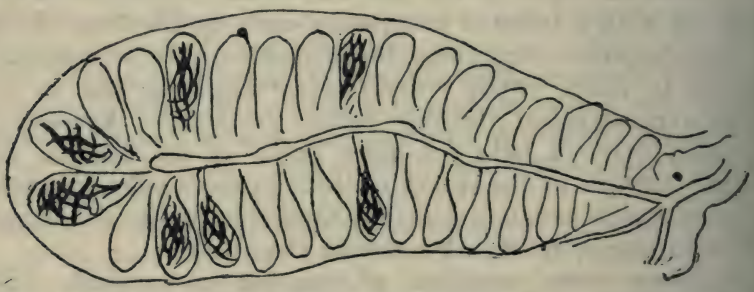
"I fancy some condition of ripening is required. This obtained I fancy the spores will infect the grubs, grow into coccidia and give rise to germinal rods and spores again. Well I am dead beat and have asked leave to go to Kurseong for a few weeks *en route* for Assam. I have lost eleven pounds in weight the last two months. Work at the microscope all night; can't sleep; and feel as low as possible."

My next letter was from Kurseong, 23 August 1898:

"I had to leave Calcutta abruptly on the 13th, much specimen making and photo taking incomplete. I had practically ceased to sleep—worked at microscope all night—and began to forget everything. Since coming here I have thought it best not to think of the germs at all. When I am in work I go too hard at it and can't shake it off my mind, with the result that I knock up in a month or two, especially in a hot climate. However the hard part of the work is over.

"I fear that you also overdid it a great deal for your health at the B.M.A. Meeting. Many thanks for your letters up to the 5th August and for copy of the Resolution of the Tropical Section, which is indeed flattering! I rather think however that they put the two paragraphs in the wrong order! The idea is of the first importance; the brain is over the hand. My function in this work has only been of the hand—that is a point I stick to. You would have polished off the whole matter in a quarter of the time I have taken. When the time comes to write the history of the whole campaign—it will have to be a separate book, not an article, I think—this will be brought into full relief, I promise you." [1]

This letter concluded with a four-page summary of my results from 4 June to 31 August 1898, including details of the infection experiments [37, 42].



GERMINAL RODS IN SALIVARY GLAND OF MOSQUITO. FROM LETTER OF ROSS TO LAVERAN, 18 JULY 1898.

I sent similar summaries to Laveran. During my stay in India I wrote him seven letters containing twenty-nine pages, dated 12 February 1896, 23 January, 25 April, 18 July, 2 September, 15 November 1898, and 3 January 1899. Those of April and July 1898 gave him all the facts about *Proteosoma*

[1] There are many prophets in Israel—but very few disciples!

in mosquitoes, with drawings of the germinal rods in the salivary gland, and the infection of healthy birds.

On 2 September 1898 I wrote:

" It is worthy to note that the grey mosquitoes which are the definitive hosts of proteosoma breed in pots, barrels and ditches containing water, even at a height of 6000 feet (2000 metres) above the sea. On the contrary, the dappled-winged mosquitoes which I believe carry paludism breed only in small puddles of rain-water where there are no fish or frogs which eat them. These facts seem to explain the telluric and meteorologic conditions of these forms of parasitism. . . ."

Years later, Laveran lent me the original letters to have copies made—which I now possess.

I left Calcutta with a heavy heart for the wretched kala-azar duty. The great treasure-house had been opened, but I was dragged away before I could handle the treasures. Scores of beautiful researches now lay open to me. I should have followed the " vermicule " in the mosquito's stomach—that was left to Robert Koch. I intended to mix the " germinal threads " with birds' blood—that was left to Schaudinn. I wished to complete the cycle of the human parasites—that was left to the Italians and others. I had designed numerous experiments on immunity in malaria and on the treatment of the disease—most of which have never been done even yet. The last mentioned research alone might have saved Britain millions of pounds during the war. No, the man who can do is not allowed to do—because the man who cannot do is put in authority over him.

On the morning of 13 August, Mahomed Bux and I released all our poor little feathered prisoners, which had performed such unwitting and unwilling service. Their numbers had been sadly reduced by mysterious illnesses, apart from malaria, and recently a mischievous hawk took to swooping down suddenly upon the cages placed in the sunshine and frightening many of the inmates to instant death. Now, when the doors of the cages were opened, many refused to go out and others even returned after going out. Some were weak and ill, but finally we got them all safely into the trees and bushes, after driving away cats and firing a shot at the hawk. May their little souls be living for ever among eternal flowers and berries in some avian heaven. Mahomed, who had given names to most of them, looked almost sad as they departed.

CHAPTER XVII

THE THIRD INTERRUPTION. ASSAM, 1898

As I have stated, one object of this book is to give the lay reader a real picture of—that rather rare thing—a real medical investigation, and of its consequences. I will therefore beg him to consider the following question: What should our nation have done in response to that memorable announcement of Manson's at the British Medical Association Meeting in July 1898? If, let us say, a geological prospector had discovered gold, or diamonds, or oil, somewhere in India, then there would have been an immediate stir; hundreds of people would have flocked to him, and the authorities would have been forced to notice and regulate the subsequent work. Or if, let us say, a hill tribe had committed some depredation, the authorities would have brought up an army against them at the cost of millions of rupees. But what was the situation now regarding malaria? It has been estimated to cause (the reader is doubtless weary of hearing it!) about 1,300,000 deaths every year in India alone, not to mention an enormous amount of sickness—almost equal to the mortality caused by the Great War; it was incommoding or paralysing most of the planting industries in India; and was, perhaps, the principal cause of unnecessary expense during all military expeditions. Now a solitary worker in India had suddenly revolutionised our knowledge (or rather ignorance) of it by showing exactly how it is communicated to us. What—unless we were to be admitted a race of unintellectual slugs—should the British authorities everywhere have done to meet this new situation? They should have sent immediately to that solitary worker, not one, but a dozen assistants, medical men, biologists, entomologists, besides subordinates, to learn his technique, to classify mosquitoes, to ascertain which of them carry the three species of human malaria, to study their habits, to determine how best, with the new knowledge, the disease may be prevented on the large scale, and *to prevent it*. Not only should the Indian Government have allotted a handsome

credit for this work, but every malarious colony in the Empire should have joined; and the work should have been commenced throughout our dominions, at least, within a year. What actually happened? My work was, not assisted, but interrupted for five whole months; one doctor was sent to me afterwards for a few weeks at the worst possible season to enquire whether I had been speaking the truth; I was forced to retire after six months, because I could obtain no definite assurance that I was to be continued at the work; and now, twenty-three years after the event, probably not one tithe of the programme sketched above has been carried out within the British Empire! It will be instructive to give the details —and possibly amusing.

As soon as the mosquito theory was proved in March–April, both Manson and I saw that immediate assistance was required—*vide* my letters of 16 and 30 May (pages 283, 285). Mr. Joseph Chamberlain was then Secretary of State at the Colonial Office, Manson being its Medical Adviser; and Lord Lister was president of the Royal Society. On 6 June, probably after receiving my letter of 16 May, Manson wrote:

"I went to see Lord Lister yesterday. He promised to go over your work and give his opinion upon it; to say whether he thought it of sufficient public importance to go with to Government and tell them they must go on with it. A week ago I saw Chamberlain, who wants investigations made on the African fevers, and who is willing to make a grant for this purpose. I told Chamberlain that important investigations were in progress in India which were about to be made public and asked him to wait for a week or two, to get Lord Lister's expression of opinion on their value before going to the Treasury for the money. . . . I will suggest a commission with you at the head of it, and will get Lister to back me up. . . . P.S. Lister was very sympathetic and encouraging."

On 24 June Manson wrote:

"On Tuesday I got Lister to come to my house, and had some 6 or 7 microscopes with your preparations for him to study. I was very ill at the time but struggled with clothes and told him all about the matter. He expressed himself convinced. I asked him if he would back me up in my representation to the Government. He said he would and was everything you could desire in his recognition of the value of your discovery.

. . . I shall suggest that the Colonial Government either make a grant for the continuation of the investigation or (agree) to send over two men to be under and assist you, and so qualify for working out the problem in Africa and other colonies. I shall say that time, a free hand, and adequate finances are necessary. . . . A permanent commission appointed with yourself at the head."

With his next letter of 1 July Manson enclosed a copy of the following open one from Lord Lister:

12 Park Crescent, Portland Place,
29 *June* 1898.

Dear Dr. Manson,

I return the copy of Dr. Ross' report with best thanks for the opportunity of reading it.

Taken in conjunction with the preparations which you kindly showed me, it seems to me to be of remarkable interest and value; and I regard it as most desirable that his experience, and I may add his manipulative skill should be turned to further account by opportunity being given him of continuing his researches on this subject.

Very sincerely yours,
Lister.

On 8 July Manson wrote again:

"I think things are going on well on this side. Mr. Chamberlain has written to Lord Lister to the effect that he wants something done in the malaria line and saying that the Colonial Government will bear half the expense. Lister called on me yesterday and I posted him again on the mosquito business so that he might be ready for the Committee meeting of the Royal Society yesterday afternoon which was to discuss Chamberlain's letter. I suggested two medical men, and an entomologist to go to India to help and learn from you, who might afterwards be sent to Africa or elsewhere to take up the enquiry. I think it will be all right now. At all events the Indian Government will see that whatever they think about the matter it is considered one of the first importance here."

Manson's scheme was admirable, especially because he hoped that the Indian authorities would thus be persuaded to place additional assistants under me for purely Indian work,

I heard nothing further about the matter for a month, but on 5 August Manson wrote: "The R.S. Proposals are not good —at all events to my mind. They want to send a research scholar to you and two bacteriologists I suppose to study blackwater fever in Central Africa. This is quite inadequate, and I went expressly to the Colonial Office to say so. The enclosed I have just received and I shall have another try for a full commission on the original line I suggested. I hope I may succeed. Your discovery is either the most important of the 'fin du siècle' or it is nothing. I shall do all I can for it on the first assumption, and try to make England work it." The enclosure referred to was a request that Manson should meet Chamberlain and Lister next day. On 9 August he wrote: "The Royal Society wished to send two biologists to Africa to study malaria. They said medical men knew nothing of biology." Finally it came to this, that Dr. C. W. Daniels, of Demerara, was nominated by Manson to come out to me, and that the rest of my "commission" evaporated into thin air, or, what was the same thing, into Africa to study blackwater fever.

In my experience almost any and every committee, even of the cleverest men, is sure to make a mess of almost any and every business entrusted to it, if it can possibly do so; and this committee paralysed malaria work in India for years. I do not know, and do not wish to know, who composed it —probably some worthy English doctors and zoologists not one of whom had made a special study of malaria or of its parasites, or knew of my difficulties, or grasped exactly what it was we wanted. Manson, who was not then a fellow of the Royal Society, seems never to have been asked to attend its weighty deliberations. Afterwards I heard that it was turned against me by two supreme faults in my "Report on the Cultivation of Proteosoma" [32]. One was that the shading in two of my drawings was hurriedly scribbled with the pen instead of being etched in parallel lines; and the other was that zoological names were not printed in italics. I hope the people of India will forgive these people some day—certainly I will never do so! Evidently the case of Galilei and the scholastics of Pisa over again.

So I was to have no assistance; but the Director-General might at least have allowed me to finish the malaria work single-handed. As already stated (page 286), I wrote to him direct on 25 May urging that it would be impossible to combine that work with such a difficult task as the kala-azar

investigation. His secretary replied on 2 June that malaria should have precedence over kala-azar; but I was not excused the latter duty, and assistance was refused; and my special duty was to conclude on 17 August. There was, however, a simple way out of the difficulty. My first small pension was due to me in June, and I could apply at once to be allowed to take it as soon as my malaria duty was to end. This would have enabled me to avoid wasting my time over kala-azar, and I even proposed to go to Italy in order to continue the malaria work there unhampered, on my way home. Such would indeed have been much the best course, if only to bring matters to a head; and I was on the point of sending in my papers in June when Manson's scheme of the commission reached me. After that, as I did not like to disappoint him, I applied on 1 July for six months' extension of the special duty. It was granted on 18 July. Meantime on 9 July I reported the infection of healthy birds and again hinted at being excused the kala-azar duty. On the 13th Surgeon-Major Leslie replied from Simla: "The D.G. desires me to say how much he is interested and how pleased he is with the success which is being obtained by you in your investigations." That was all. Nothing about being excused kala-azar. So I was doomed! For the second time I was to be interrupted just as I had the conclusion of the human malaria work within easy reach. Already much wearied with the old task, I was to undertake a new and utterly different one which would occupy me for months. Columbus having sighted America, was ordered off to discover the North Pole! I left Calcutta on 13 August, and a week later heard that Manson's scheme for a commission had collapsed.

The reader will not be long detained over kala-azar. It is a fever; not intermittent like malaria, but one which continues for months, with long waves of improvement or exacerbation during which the spleen and usually the liver become enormously enlarged and the patient thins to a living skeleton—somewhat like ankylostomiasis (hook-worm disease) on one hand and chronic malaria on the other. But the malady was worse than either; whole families, villages, and coolies' quarters were affected, and it was a dreadful sight to see the poor wretches, nothing but skin and bone, with enormous abdomens, children and adults, trying to keep alive by lying in the sunshine—and death was then almost inevitable. It is sights like these which make the medical investigator.

Kala-azar (*black sickness*) was first mentioned in the *Annual*

Sanitary Report for Assam, written by Dr. Clarke, for 1881, page 28; and in the similar Report for 1882 a description of the disease is given, based on a report by Mr. McNaught,[1] then Civil Medical Officer at Tura, Garo Hills, Assam, who studied 120 cases of the disease, and the epidemiology of it, and who discovered the malady. As it spread in Assam, most of the medical officers thought it was only a very virulent form of malaria; but others doubted this, and in November 1889 Surgeon-Major G. M. Giles, I.M.S., was put on special duty to report upon it. He concluded that it was hook-worm disease; but as the local medical officers resisted this opinion also, the Government sent Surgeon-Captain Leonard Rogers, I.M.S., in April 1896 to make further investigations and he concluded that it was malaria. I was now required in 1898 to decide, and ended by supporting Rogers (with some amendments). We were all wrong. The disease is one of another class, which exists in many parts of the world, Old and New, causes many kinds of illness, and is due to various species of a new genus of parasites called by me in 1903 *Leishmania* [74]. The difficulty of my task may be gauged from the fact that most cases of kala-azar are *also* heavily infected both with hook-worms and with malaria, being cases of what I called *Herodiasis* or Multiple Parasitism, and that at that time we did not generally use in India the Romanowsky method of staining, which is almost essential for distinguishing the *Leishmania*. Even Manson scarcely recommended it in the first edition of his *Tropical Disease*, 1898, page 31. I *ought* to have used it: if I had done so I should doubtless have found the *Leishmania* myself, besides being able to stain my "proteosoma-coccidia" and "germinal threads" better than I had done; but I had really had no time to study new staining methods while in the midst of making my fundamental "differential experiments."

Just as I was about to leave Calcutta for Assam I heard that Captain E. Harold Brown, M.D., I.M.S., had recently been investigating an allied disease of Bengal called kala-dukh (*black or deadly pain*); and that there was a similar sickness in the Darjeeling Terai called either kala-dukh or kala-jwar (*black fever*). The latter area was close to Kerseong, where my wife and family still were; and I obtained permis-

[1] I believe that Mr. McNaught was then only an Assistant Surgeon. He was given commissioned rank later and was a Captain I.M.S. at Nowgong when I was there. I cannot find his name in Colonel D. G. Crawford's *History of the Indian Medical Service* (London, 1914), and he seems never to have received any acknowledgment for his most important discovery.

sion to go there first. Leaving Calcutta on 13 August 1898, wearied out with heat and work, I spent a few days at Kurseong doing some odd jobs, and on the 24th proceeded with Mahomed Bux and Lutchman to a place called Naxalbari, about nine miles out on the plain, supposed to be the centre of kala-jwar. We lived there with some very kind planters (Mr. Longmore and friends), who, like Horace, scorned mosquito-nets, and had constant fever, but who solaced themselves by playing beautifully on the violin ! About this visit I wrote :

KURSEONG, 6 *Sept.* 1898.

DEAR DR. MANSON,

I spent a week at Naxalbari. Results poor. In about 100 persons examined malaria parasites were found only in 5 or 6, though numbers of the cases had enormous spleens with a kind of hectic fever. The cases were evidently old malarial infections. As the people always refused to stay for repeated examinations motions could not be often looked at for eggs. Not a single suitable case for mosquito feedings was obtained.

The mosquitoes were dapple-wings and greys. The former breed in rain-puddles exclusively I think. Hence, the whole climatic variations of æstivo-autumnal. I examined numbers of both kinds caught in houses of fever cases. No coccidia or rods. As however the fever cases in question had only a few wretched young parasites in a whole specimen infection of mosquitoes was unlikely. I did not see a single crescent the whole time (remember what Rogers said). No rods were seen in the salivary glands of many mosquitoes. The dapple-wings bite very fiercely. Their grubs float horizontally like sticks in the puddles, a noteworthy and very useful peculiarity.

I don't believe that fresh malaria was about at all at Naxalbari at this season. There were floods of rain which continually scoured out the puddles. People said that there were very few mosquitoes. Dapple-wings won't live in any but the smallest puddles because the minnows which swarm in anything over a few gallons capacity devour them instantly. Malaria is probably a *very* easily prevented disease. I preached mosquito-nets and an anti-puddle propaganda and left. Tomorrow I am off to Assam. . . .

The real Terai disease is probably anchylostomiasis and not

malaria. Many frightful cases of the former. Hospital assistant at Naxalbari had never *heard* of it ! The worst cases of mere malaria with spleens touching livers looked quite cheerful and jolly.

Filaria is also present you will be *glad* to hear. I made the following most interesting observations which you may like to add to your filaria lore. While examining mosquitoes I took some grey ones caught in a mosquito net within which a native catechist, his wife and one child, all with fever, slept nightly. The thorax was seen to contain something new. It was a development stage of f.s.h.[1] Numerous forms of all stages up to the full active stage were easily found in all of 5 mosquitoes examined. Evidently our catechist was too lazy to kill his mosquito in the morning. Taking his night blood I found F. nocturna. A case of filaria diagnosed through the mosquito !—and a very easy way of diagnosing it too. I shall employ it clinically some day.

Have had a great disappointment. P. D. Simond [2] has been here. I wrote and he came down to see me. I had just gone to Naxalbari. He followed me down the line ; but it was no use, he had to be in Calcutta on a fixed date and we did not meet after all. I would have given much to have shown him the work, and find out something about coccidia.

I have had a nice letter from Nuttall.[3]

Have written officially to Government and asked for permission to publish. The present state of things is very annoying.

I thought you would not find the Royal Society rise easily to your fly. . . . I shall be in Assam in a few days. Don't expect to be able to do anything at all with Kala-azar at this season. All the malaria cases will be probably stale ones—no parasites—nothing decisive. But I shall make every effort to bring at least one of the human parasites into line with proteosoma. (Am very apprehensive about not finding crescents.) This done (with luck it may be done in a week or two), if kala-azar turns out unfruitful, I may come back to Calcutta by 1st Nov. where I can demonstrate to Daniels and the other man. . . .

[1] *Filaria sanguinis hominis.*

[2] Of the Pasteur Institute, who had written on the flagellate forms of the *Coccidium oviforme* of rabbits, page 251.

[3] See page 334.

I see by the *B.M.J.* for August 20th, p. 498 that Koch has gone to Italy to study malaria. It is quite possible that he will at once succeed in infecting men by the mosquito if he is lucky. In the meantime I am absolutely forbidden to publish anything about my infection of birds. . . .

Yours sincerely,
R. Ross.

The first part of this letter indicates my malaria doctrine at the time, including the attitude of *Anopheles* larvæ, their breeding habits, their destruction by fish, and the prevention of malaria by protection and mosquito-reduction—all of which have been frequently discovered since then quite *independentemente da Ross*, of course! I find from my notebook that altogether ten [1] dapple-winged mosquitoes (caught in one of Mr. Longmore's go-downs, in which there was a sick servant) were examined on that occasion. By bad luck all were negative. They were mostly *Anopheles listoni*, in which Stephens and Christophers, with better fortune, easily found zygotes later in this very district.

Leaving Kurseong on 9 September I travelled by train and steam ferries to the bank of the Brahmaputra near Jatrapur, whence we took river steamer on 10 September. The great river was in flood—not in a trench between steep banks like the Irawadi, but a vast islanded expanse of swirling brown water up which the steamer panted slowly. We (Mahomed, Lutchman, and myself) reached Gauhati in Assam on the 11th, and, after seeing some cases there, arrived at Nowgong on the 13th. This I described to my wife as "not at all a bad little place. A pretty full-fed river winds along in front of the station, which is built on flat ground with large trees between the houses . . . will you send me my fishing things?" It was now the centre of kala-azar, which had moved hither from the Garo Hills, and Dr. McNaught himself was in charge of the hospital. I lived most of the time with Mr. W. J. Reid, I.C.S., the District Magistrate—an able young officer, who was extremely kind to me and helped me in every possible way. I remained there, examining numbers of cases, doing post-mortems, and taking depositions, until 21 October, after which I moved, with Reid, to Mr. G. Jamieson's bungalow not far away. This was the place where a tiger had walked in one night and taken out a planter by the hand (between his teeth) in order to eat him fresh outside!—the planter was rescued

[1] This is the correct number. I said "some dozens" in my Nobel Lecture.

but his hair had turned white! Thence I went to Umluki, Silghat, and Tezpur, in order to take the depositions of Drs. Lavertine, Dodds-Price, and Macnamara, all men of great experience with regard to kala-azar. On 26 October we took boat again at Gauhati, and on the 28th reached Kurseong again, where I commenced my report on kala-azar.

Apparently the reason why Giles considered kala-azar to be hook-worm disease was that he never met McNaught and was presumably never shown true cases of kala-azar. Directly I arrived at Nowgong and was shown cases by McNaught, I concluded at once that they were not hook-worm disease, and told Manson so in my letter of 9 October. The only question now was whether kala-azar was merely malaria or something quite different. The early stages were indistinguishable by me then; and no wonder, because most of them had the double infection. But later the kala-azar cases showed an enormous enlargement of the spleen and liver, with continued fever which were, at best, not common in simple malaria. On pricking these organs in living patients at this stage, and also in half a dozen autopsies, I seldom found malaria parasites or pigment, and hence might have rightly but rashly concluded at once that they were not malaria. Indeed this was my opinion for some weeks. But there was another possible explanation of this absence. The cases may have been originally due only to severe malaria, which had gradually died out of them owing to the establishment of personal immunity, leaving them wrecks with permanently damaged spleen and liver. How could this happen? The medical dogma of the time was that there was no immunity in or against malaria; but I saw at once, as the logical result of my findings in July, that there must be immunity in malaria (quite apart from the kala-azar question). I think this was first announced by me in my letter to Manson of 16 October from Nowgong:

"Now why does the parasitic invasion cease after a time? Certainly not in consequence of quinine because these people don't take it. This is my theory. *Immunity* is established. Compare my results with the reinfection of sparrows. Probably these kala-azar cases become infected over and over again in the early stages by mosquitoes which have bitten themselves previously—or their diseased relatives—and they would simply perish straight off unless some process of immunity were established. This is established, but the organs are left wrecked.

"Hence kala-azar at the stage when it is usually diagnosed as such is in reality no longer a hæmamœbiasis curable by quinine, but a kind of spleen and liver disease with the 'fièvre symptomatique' of Kelsch and Kiener and yellow pigment only. The main incidence falls on the liver.

"The main point to fix the mind on is that in these severe untreated cases of malaria a species of immunity is established in a few months, which enables the system to eliminate the parasites itself, but only at the expense of profound siderosis and often ultimate destruction of the liver and in a less degree of the spleen. Compare Kelsch and Kiener's cases of 'Chronic malaria.'

"Among Europeans who treat themselves with quinine and are not nearly so exposed to mosquito infection and reinfection this immunity is seldom established so that they frequently have attacks of hæmamœbic fever to the last, giving, of course, the usual black pigmentation."

Again, on 15 November I wrote to him from Kurseong: "My solution is that immunity against the parasites is established after a month or two, after which the black pigment is eliminated, but the pathological condition of the organs (due possibly to the yellow pigment) remains. The disease must be malaria as every gradation from early cases with parasites and (black) pigment in the spleen to established K.A. cases without either, existed." Nevertheless I was wrong!—but the theory of immunity here indicated still holds for malaria in general.

It was another medical dogma of the time that the black malarial pigment is never eliminated when once deposited in certain organs. This had been completely disproved by my recent observations on birds: more pigment was often found in a bird which had been infected only for a few days than in one which had been equally heavily infected for a month.

When Rogers said that kala-azar was malaria, he was disbelieved because of a third medical dogma, that malaria is not communicated from man to man as kala-azar is. This, of course, had also been disposed of by my results of July.

When I was sent on this wild-goose-chase, neither myself nor, I believe, the Director-General ever dreamed that, apart from the investigation itself, the actual writing of the Report would occupy me for months; but this was the case. The necessity of discussing all these points, of examining innumer-

able pathological details, of consulting authorities (none too sound), and of reconsidering the subject with Dr. C. W. Daniels (who arrived before Christmas), compelled me to waste upon it nearly the whole of my time from the end of October to the end of January 1899. The Report [44] covers nearly ninety foolscap pages of close print. I was tired when I commenced it; gradually became so fatigued that I could scarcely write a page a day; and ended by being utterly "fagged out" by it. Yet all this precious time and labour might have been spent on finding the carriers of the human *Plasmodia* and in finishing, describing, and figuring properly my work done in July and August, my observations on mosquitoes, and my full technique.

Of course, owing to the press of pathological work, serious mosquito work was impossible at Nowgong, but I fed fifteen dapple-wings (*mostly A. rossi*) and eleven Elephant Mosquitoes (*Panoplites sp.*) on a case with a few crescents and on another with quartan malaria. All were negative, but the feeding was left to assistants. I also examined a few insects caught in the houses of patients.[1]

During my stay at Nowgong I wrote for Government a short Preliminary Report on the Infection of Birds with "Proteosoma by the Bites of Mosquitoes" [37]. It gave little more information than Manson gave at the British Medical Association meeting in July [34].

I think I saw the *Leishmania* several times in the small quantities of spleen-pulp which I often extracted from patients by means of a hypodermic syringe—on payment of a *douceur* of two or three annas; but I cannot be sure because the stain was not used. This little operation, which was almost painless and quite harmless, was often the source of some amusement, because after a time the children almost demanded it. One boy called Pual said it made him feel so much better; perhaps this was due to the sweets which he bought with the money!

On 18 November we all left beautiful Kurseong for Calcutta, where we had been obliged to take an expensive flat at 31 Chowringee. After entering it we soon discovered that empty tin packing cases were being hammered into shape all day long in the backyard of a neighbouring shop—good for writing my Report! My laboratory was ready for me, but alas! I had no time to use it. There we remained until my

[1] Nowgong was full of *Panoplites* with their extraordinary egg, like a kind of tobacco-pipe. I first saw and figured these in Calcutta on 31 March 1898 (page 275), and afterwards showed both eggs and mosquitoes to C. W. Daniels.

period of special duty terminated (16 February 1899), and we left India for good.

I see by my notebook that we started work again to some small extent on 25 November. But the conditions had now changed: comparatively few of the wild birds were infected and the mosquitoes refused to bite in the dry cool air of that season in Calcutta. We had some old incubators in the laboratory, but the smell of the gas, or something else, offended our fastidious victims.

During all my stay in Calcutta, scarcely anyone except Dr. Dyson, I.M.S., the Deputy Sanitary Commissioner, and Dr. Cooke, the Health Officer, had ever troubled to see my work, though the city was full of doctors and professors. But in October, when I was in Assam, Dr. S. W. Rivenburg, of the American Mission there, wrote and asked permission to join me in Calcutta. He was quite unknown to me, but arrived with his wife and children (at his own expense) on about the 26 November. Of course he came to learn; but he also helped; and I respected him much for his enterprise.

On 22 December the long-expected succour from the Royal Society arrived at last in the shape of Dr. C. W. Daniels, of British Guiana. I had particularly wanted an entomologist to classify my mosquitoes; but Daniels was not one, and, moreover, could not help me with the Romanowsky stain. He had, however, a great experience of malaria, a knowledge of its pathology, and a sound scientific judgement (short of mathematics); and he assisted me greatly in preparing my "Kala-Azar Report." On arrival he was polite but cold, and evidently sceptical even of my old *Proteosoma* results. In fact he had not been sent to assist me, but first to ascertain whether I had been speaking the truth, and secondly, if so, to "suck" my brains! The unfortunate committee which sent him might have saved itself the expense because dozens of specimens proving my case were in Manson's hands in London at the time, and my technique had already been sufficiently described in my "Report on the Cultivation of Proteosoma." But nevertheless we welcomed him, and he was soon convinced. Later on, he and Rivenburg succeeded in infecting twelve out of twenty-two healthy sparrows by the bites of *Culex fatigans*; but, though we at last procured two good cases of crescents and used incubators, we failed to infect any "Dapple-wings." There were two reasons for this: most of these mosquitoes were *Anopheles rossi* Giles, now known to be negative to malaria, and, secondly, the nights were cold

and we had no warm chamber. For this latter reason, the transmission experiments of *Proteosoma* also often failed, the pigmented cells developed much more slowly, and a much smaller proportion of wild sparrows were found infected. Our bad luck clung to us ; but I was too busy with my Report to assist much.

On 23 January 1899, Daniels forwarded a report on all this work to the " Malaria Investigation Committee " of the Royal Society, which had sent him to me ; and in this he confirmed every detail of my previous findings. By then, however, the matter had lost interest because my work had already been pirated by the Italians. For some reason best known to itself the Royal Society did not publish Daniels's report until 6 July 1900 (Proceedings). On 12 February he left me to join the other members of the Royal Society's malaria commission in Africa.

All this time plague had been gathering force in India, and it still hampered us up to 1899. The authorities had fortunately been saved from the results of their previous want of preparation by possessing in Bombay the services of a man of genius, Mr. W. M. W. Haffkine, who had come to India to popularise his anti-cholera vaccine, and had then invented his similar prophylactic against plague in 1916. But instead of giving him all the help he needed, they now appointed the usual commission " to examine and report." It consisted of Dr. T. R. Fraser, an Edinburgh professor, who had done good work on the cure of snake-poison, Dr. Almroth E. Wright, Professor of Pathology at Netley, who had invented the important prophylactic against typhoid, and Dr. M. Armand Ruffer, a bacteriologist in Egypt. The two latter gentlemen visited Calcutta about Christmas 1898 and were, I think, sent to inspect my work " privately." We arranged to meet one morning in my laboratory, but as I was a few minutes late they asked Daniels to show them the " pigmented cells." Unfortunately he was not yet fully acquainted with them and showed instead the nuclei of the cells of the mosquito's Malpighian tubules. Ruffer detected the error at once, and thought I was an impostor ; and when I arrived there was almost a fracas ! A specimen or two calmed it however, and Wright and Ruffer ended by accepting my work fully—except the " black spores."

I have explained (page 310) what trouble those bodies caused me. Often I thought they were only parasites of parasites, but at other times, up to January 1899, I was almost convinced that they were really spores of the *Plasmodia*, though I failed to get them to develop. Now Drs. Wright and

Ruffer supported the former hypothesis, and Daniels the latter one; but before the end of the month I found numbers of the bodies in mosquitoes which had never been fed on infected blood at all, and therefore expressed grave doubts as to their nature on the concluding page of my "Kala-Azar Report" (30 January 1899). I was quite right to spend time over the question, as it was not one to be summarily dismissed, and has not been fully elucidated even yet. The subject of parasites of parasites is important apart from malaria. My work on kala-azar enlightened me on one point: the epidemiology of malaria could be fully explained on the supposition that the disease is communicated only from man to man by the mosquito —it was not necessary to assume further that mosquitoes infect each other and that they rise from the marsh already infected, as King had supposed (page 124). Hence the "black spores" were no longer necessary in the life-cycle of the *Plasmodia*.

Of course we showed Daniels all our technique and gave him all we knew about the habits of our mosquitoes—numerous facts, indeed, which I never published owing to my frequent interruptions. Of course all this was subsequently attributed to others who had really merely learnt from me; and Daniels consequently gave me the following letter upon the subject.

Letter from C. W. DANIELS, M.B., *Member of the Malaria Commission of the Royal Society and the Colonial Office*

8 *October* 1900.

DEAR ROSS,

I shall have great pleasure in testifying to the following facts:—

(1) Shortly after my arrival in Calcutta in December, 1898, you showed me living specimens of your "grey," "brindled," and "dapple-winged" mosquitoes.

You pointed out to me the attitude assumed by the last, the position of its larva in water, and the peculiarities of its eggs.

Since then I have learnt that these are characteristics of the genus *Anopheles*.

You contrasted these with the eggs and larvæ of other mosquitoes, which I now know to belong to the genus *Culex*.

(2) You showed me two species of "dapple-winged" mosquitoes, and I sent specimens of them to the British Museum

where they now are. They have been described by Major Giles, I.M.S., as *Anopheles*.

You also showed me a specimen of the stomach of a mosquito with what are now known as " zygotes." This you stated was the stomach of a " dappled-winged " mosquito, similar to those you had shown me, which had been fed on a patient with crescents in 1897.

(3) I may add that the development of Proteosoma, as demonstrated to me by you in the " grey " mosquito, is essentially the same as the development of " crescents " in *Anopheles* as observed by me in British Central Africa.

I am, yours very truly,

C. W. DANIELS.

P.S.—As regards breeding-places, you informed me that the " dapple-winged " mosquitoes breed in puddles, the " brindled " mosquitoes generally in flower-pots, and the " grey " mosquitoes in tanks, ditches, etc.

C. W. DANIELS.

MAJOR R. ROSS.

All these points are of fundamental importance in the theory of mosquito-borne diseases. About the same time the same facts were demonstrated by us to Dr. F. Plehn, a good German worker on malaria who spent several days at the laboratory. I do not think that I ever received a paper by him acknowledging this help; but certainly Daniels and he spread the cult widely from that time, since when everyone who ever saw a mosquito was able to rediscover the same facts for himself. To such the discovery of the facts is usually attributed in our learned textbooks on the subject. The plague-scare was still so much in evidence that the colonel of a native regiment refused to allow Daniels, Plehn, and myself to examine his men.

During my year's special duty I received some, but not many, letters from people in India regarding my work. Among these were letters of congratulation from my old friends, Surgeon-Colonel Dobson and Surgeon-Major John Smyth, then both at Bangalore. While I was still at Kherwara, Dr. Hayman Thornhill, Senior Medical Officer of Ceylon, a real enthusiast on these subjects, offered to send me some living

dapple-winged mosquitoes all the way from that island; but we decided that the idea was impracticable (at least, so far as I remember). Dr. G. Thin, of 63 Harley Street, London, wrote me several pleasant letters in which he gradually dropped his mistaken estimate of the pigmented cells (page 253), though he never withdrew publicly his rather mischievous error. In the April number of *The Indian Medical Gazette* I had called attention to the need for a circulating medical library in Calcutta (fancy there being none !) ; and I have an interesting letter from that indefatigable and successful worker, Captain (now Sir) Leonard Rogers, I.M.S., on the subject; and another later one on kala-azar. I have several letters from Dr. Maynard, who helped us by publishing our papers in *The Indian Medical Gazette*, of which he was editor, though I doubt whether he fully believed in "Laveranity." I have three long ones from Surgeon-Major G. M. Giles, I.M.S., who was then at home on furlough. He was courteous, but entirely sceptical of the mosquito theory on rather feeble grounds, being evidently a good observer but a bad reasoner. My replies (copies of which I regret I do not possess) seem to have converted him, for some years later (1900) he produced his *Handbook of the Gnats, or Mosquitoes* [54], which became the basis of malarial entomology. In this book he scarcely mentions my work except, very kindly, to apologise for my inaccuracies (2nd edition, page 153). When I met him some years later I asked him what precisely these inaccuracies were, but found that he had never read my papers—or at least understood them ! It was he who named *Anopheles rossi* after me, because he "thought" they carried human malaria, though Daniels told him they did not. Nevertheless Giles was a valuable man, and I wish I had had him with me in 1898. He never received a thank-you for his work. He mentioned my work as little as possible in his book, and, *more Anglicano*, favoured the foreigners.

In June 1898 I had sent a copy of my *Proteosoma* Report [32] to Surgeon-General Sternberg, the Director-General of the Medical Department of the United States Army, and had written to him later giving my further results of July and August. I have no copy of my letter, but the reader may like to see the reply now in my possession, which proves that I had even then told the Americans how to reduce malaria by the methods which one of Sternberg's officers, W. C. Gorgas, employed so successfully some years later in Havana and Panama.

Subject: Causation of Fevers.

WAR DEPARTMENT, SURGEON-GENERAL'S OFFICE,
WASHINGTON, *October* 14, 1898.

SURGEON-MAJOR RONALD ROSS,
Indian Medical Service,
Care Postmaster, Calcutta, India.

SIR,

I am directed by the Surgeon General to acknowledge receipt of your interesting communication of August 24th last, in relation to the causation of malarial fevers and to the practical deductions from your observations bearing on the suppression of such fevers by the use of mosquito nettings and satisfactory drainage.

No report has as yet been received from any of our medical officers on the causation of the fevers which affected our troops in Cuba and in the southern Camps. Their energies so far have been wholly engaged in caring for the large number of sick men. Should any report be received bearing on your views, the Surgeon General will have pleasure in sending you a copy.

Very respectfully,
(?) C. STUART,
Deputy Surgeon-General, U.S.A.

This letter was dated before a single person in the five continents had been able to repeat a single one of my observations on malaria and mosquitoes!

While I was in Nowgong permission at last reached me to publish my results (page 325). The letter was addressed to my Chief from the Government of India, Home Department, 27 September 1898, and said that "The Government of India have no objection to Surgeon-Major R. Ross giving publicity to any results of his investigations connected with Malarial fever after they have been duly communicated to you." Yet my Chief had declared that the assent of the Secretary of State in London was required! All the trouble over my *Proteosoma* Report had therefore been, as my friends told me it was, quite unnecessary. But for Manson's expositions, I might have lost priority for all my work on avian malaria. The letter mentioned above was really a snub for Leslie, who, I think, caused the trouble.

CHAPTER XVIII

THE DISCOVERY DISCOVERED. CALCUTTA, 1898–1899

THE lay reader who is not acquainted with the niceties of scientific priority may be surprised that I should insist so frequently on mine. He will know the reason soon, because I have now to describe the discovery of my work in Europe—*indipendentemente da Ross.*

As stated on page 288, my *Proteosoma* Report [32] had been sent out in June 1898 to a number of people—Manson, Laveran, Metchnikoff, Thin, Giles, Sternberg, the President of the Royal Society, the medical press, and elsewhere—about thirty or forty copies. This report described the necessary technique, and figured the pigmented cells completely up to, but not including, sporulation. The rest of the life-cycle was described in detail by Manson before a large audience of the British Medical Association at the end of July [34], all the facts being given in *The Lancet* of 20 August, while Manson's full paper, with a number of illustrations, including some of the germinal threads in the salivary glands of mosquitoes, appeared in *The British Medical Journal* of 24 September 1898, of which many thousands of copies were issued. Besides all this, I had sent scores of my specimens and mosquitoes to Manson and to Laveran; and on 7 October the latter had written to me from the Pasteur Institute of Paris—the recognised world-centre for such studies —"Je vous remerce sincèrement de votre très intéressante lettre du 2 Sept. qui confirme et complète, sur plusieurs points importants, vos précédentes lettres. . . . Les résultats des infections expérimentals, obtenus chez des oiseaux sains, sont très remarquables; je vous félicite d'avour résolu ce difficile problème." After this, there would be no trouble whatever in repeating the same work for any of the parasites, human or avian; and how anyone could have the impudence to pretend to do so independently of me would be a mystery outside pure charlatanism.

How easily the work could be repeated, given time and opportunity, was proved in August 1899 when, within a few

days of landing in Sierra Leone, I was able to demonstrate the development of all three species of human *Plasmodia* in two kinds of " dapple-wings," *Anopheles costalis* and *A. funestus* (page 380). On 6 September 1898 (as already quoted) I had told Manson that it would be easy to infect healthy men experimentally by the bites of mosquitoes by my method, and on 31 October I wrote :

" You must not trouble yourself in the least about obtaining confirmation of my mosquito work. Before another year has passed there will be dozens of workers all over the world. There is really no difficulty at all *except finding the right hosts* for each species of parasite. A few years more and the whole business will be established as the most beautiful of all the processes of nature in connection with disease. Malaria from beginning to end will be the ' show ' disease."

All this happened. My storm-tost treasure-bark had now safely crossed the wide solitary ocean and was approaching harbour ; but it was soon to be surrounded by a clamour of pilot-boats and steam-tugs all offering to navigate me in (on payment), and the pirates lay in the offing ready to board me at the proper moment !

The current facts were supplied to me in two series of amusing letters from Dr. G. H. F. Nuttall and Dr. Edmonston Charles. The first man to venture forth was the great Robert Koch, of Berlin, the discoverer of the cause of tuberculosis and of cholera, who first established bacteriology as a practical science by his plate-culture methods. On 2 September 1898 Manson wrote to me :

" Koch has gone to Rome to study Malaria. Pity he didn't go there before. I sent him one of your reports which the Indian Office sent me. . . . I also learn officially that Koch and a commission are going out to India [and] Africa [illegible] for 2 years to study malaria. . . . Doubtless your mosquito discoveries have instigated this move. In his later lucubrations Koch claims the mosquito as his own and never says a word about your work. Cool is it not ? "

On 6 October Manson wrote to me :

" A great many of your slides have perished utterly, some are deteriorating rapidly. A good many are still in good

preservation but I fear for their future. I am sending one to Rome in a day or two. I hear that Bignami and Marchiafava are going into the country to work mosquitoes and I also hear that the American pack is in full cry after the mosquito."

On 30 October he wrote: "I hear Koch has failed with the mosquito in Italy so you have time to grab the discovery for England"—he meant to complete human malaria in the mosquito. Similarly Laveran, in his letter of 12 September, told me that Koch admitted malaria infection by mosquitoes, but that he had not added more evidence in favour of this. I did not know of Dr. Nuttall until I received his first letter. He said:

ALTONAIRSTRASSE, BERLIN, N.W.,
13 *August* 1898.

DEAR SIR,

. . . Whilst in London lately I called on Dr. Manson, who was very kind, and we spent an evening over your preparations, one of which he has since kindly sent me. I have just written a review of your remarkable researches for one of the German journals—the *Hygienische Rundschau* feeling that people here ought to know more about them. Dr. Manson sent me your "Report on the Cultivation of Proteosoma Labbé, in Grey Mosquitoes," 1898 Calcutta, and in a letter has told me of your last beautiful observation of the parasite in the Salivary Glands of the mosquito. . . . I am at present engaged in a lengthy paper in which I give a critical and historical review of our present knowledge of the part played by insects in infectious diseases. . . . I shall be publishing it in English as well as in German, and shall have the pleasure of sending you a copy in due time. The mosquito-malarial theory (?) will be discussed at length and your work too. Koch who came back recently from German East-Afrika declares himself in favour of the theory and says it would be very important to follow up the matter experimentally on mosquitoes. He ignores your work completely. I have just written a review of his publication and have drawn attention to this. . . . Hoping that you will reap the reward of your brilliant and painstaking researches in the shape of suitable encouragement from the Government, I remain, my dear Dr. Ross,

Yours very sincerely,

GEO. H. F. NUTTALL.

He also made me some suggestions which I had already considered. I did not like to hear all this of Koch, who was to me a star of the first magnitude, and I could not conceive how he or any man of fame could plagiarise the work of an obscure person like myself; but it was evident that scientific morality was not as impeccable as I thought. When however Koch's article [50] appeared in 1899, he gave me full credit in a single sentence, though he did not mention Manson's theory.[1] He confirmed all my *Proteosoma* work, and described the movements of the "vermiculi," which I had no time to examine; but he did not deal with the human *Plasmodia*. He had thought of the mosquito theory as early as 1883–4, and now cultivated *Proteosoma* in Rome in September 1898 —see his letter to me on page 73 of [78].

Nuttall's subsequent letters to me gave chiefly several criticisms of my nomenclature and some suggestions, one or two of which would have been useful if I could have continued my work. I explained that my terms such as "proteosoma-coccidia" and "germinal threads" were, like "grey mosquito" and "dapple-winged mosquito," merely provisional, as I could obtain no scientific advice on the subject in India.

Now for the Italian work. As already quoted, Manson told me on 6 October that he was sending one of my specimens to Rome. He sent two to Dr. T. Edmonston Charles, a retired Deputy Surgeon-General (? I.M.S.), who was living there. After this Drs. A. Bignami and C. Bastianelli and Professor B. Grassi of that city gradually carried out for Italy the local researches which I had always considered would constitute the necessary second part of my investigation; that was, to extend my general results to local parasites and mosquitoes; but they were able to do so only in the light of my work, and not, as they pretended, independently of me. Seven months later I did the same for Sierra Leone—and would have done so had Koch, Bignami, Bastianelli, and Grassi never existed. The reader who is jealous for the honour and welfare of science, as I am, should follow the facts. I will give here only some of them,

[1] His exact words were:
"Die Erforschung der Malariaätiologie hat drei wichtige Entdeckungen aufzuweisen:—
"1. Die Entdeckung der Malariaparasiten durch Laveran;
"2. Die Entdeckung des Entwickelungsganges der Malariaparasiten innerhalb des eigentlichen Wirthes, d.h. im Körper des erkrankten Menschen durch Golgi und
"3. Die Entdeckung der Entwickelung der Malariaparasiten ausserhalb des menschlichen, bezw. thierischen Körpers im Zwischenwirth, in der Stechmücke durch Ross."

as they became known to me (see also Chaptes XXI). The piracy has now become merely ludicrous; but nevertheless it is my duty to expose it once more in this book.

21, Queen Anne Street, Cavendish Square, W.,
8 *November*, 1898.

My Dear Ross,

Charles writes me he has given you the great news from the Imperial City. You are now on the mounting wave which I trust will surely lead on to fortune. Bignami should have experimented on your lines though. He ought to have fed his mosquitoes on crescent blood, waited a week for the germinal bodies to form and then set them to bite. I have written Charles to tell him this and I hope he will not be too late this season to try. I hope however you have anticipated him and that you can give chapter and verse, that is mosquito and experiment for tertian quartan and crescent malaria by this time. . . .

I sent Bignami and Charles one of your preparations each. I grudge to part with them but I think it is in your interest that the Romans should have ocular proof that you have been before them. I fear that these furrineers are inclined to filch as much as they can. This is a sentiment I only breathe to you. We will take care that your interests are looked after here. . . .

I hope you are feeling better and that you are able to place the top stone on your magnificent building.

Yours sincerely,
Patrick Manson.

Dr. Charles's letters were all type-written.

72 Via di San Niccolo da Tolentino, Rome,
November 4th, 1898.

Dear Dr. Ross,

Dr. Crombie stayed with me at Upper Norwood this summer, and I went north to Glen Urquhart to stay with him. Naturally we discussed your work much, and Crombie was to have given me an introduction to you, but the second visit he intended to pay me did not come off, and I had to leave London without any letter for you, and for the moment I do not know Dr. Crombie's address.

I called on Dr. Manson before leaving London to get the latest news of what progress you had made in your work, in order to let the Italians know. They have been working in various directions this summer, but up till this week without being able to show any definite results. Bignami has collected mosquitoes from four very malarious localities.

According to Grassi, it would seem there are some fifty varieties of mosquitoes in Italy. Only six, however, seem to frequent these selected malarial positions.

Besides the mosquitoes, the larvæ were also brought up, and allowed to develop in Rome.

Four different persons were shut up again and again in a room with a number of these mosquitoes. The experiments extended over months, till at last a success has just been met with.[1]

Besides these experiments, Bignami has been studying the question of the Flagella with great care.

As he has found Chromatin in some but not all of the Flagella [2] distributed irregularly, he will soon have to make up his mind to abandon the position he took up, that only degenerated forms of the Plasmodium gave rise to flagellation.

Celli has been working at the subject of rendering men immune to malaria. He has been using the serum of immune animals and extract of the internal organs. His results have not been published, but there is an idea abroad that he has met with partial success.

Koch is not in Rome now, but though I tried hard to find out what he had been doing I can get hardly any information.

He seems to have been very silent as to his work, and to have refused to speak about it. It may interest you, however, to know that he spoke in the highest terms regarding your work, and also that he looks upon the Flagella as being some kind of spores.

My friend, Dr. Brock, looks upon the movement that he sees in the pigment of the body before flagellation takes place as indicating the Flagella are moving about before they become extruded.

By the same post I send you a copy of a lay paper, *The Tribuna*, bearing today's date.

In the article I have marked, it is stated that Grassi holds

[1] But only after change of procedure (page 340).

[2] This had already been done by N. Sakharoff [15].

that only three species of mosquito are capable of conveying malarial fever. It is also stated that one of Bignami's patients developed fever for two days after having been bitten by these mosquitoes, and that yesterday, for the first time, malarial parasites were found in his blood. Immediately after this the patient took quinine.

From my conversation a week ago with Bignami I satisfied myself that if he got any successes we could rely on his careful experiments as excluding error. I, for one, therefore, accept his position without hesitation. I have been three times to-day to try and catch him at his laboratory, but was unsuccessful in finding him. I will go again, however, before the post leaves, in case any other patient that he has been experimenting with may have developed fever.

In *The Tribuna* it is mentioned that Dionisi has found in bats malarial infection caused by parasites similar to those of human malaria. This observation of his has still to be followed out.

Although I have sent you *The Tribuna*, unless it is easy for you to have it translated do not do so. I have picked out all the points that will interest you.

I have told Professor Marchiafava that I will write and ask you for specimens of the mosquito in which human malaria develops.[1] Also for those which you use with regard to bird malaria. They will send them to Grassi to be identified with the Italian mosquito.

I suggested to Bignami that they might be sent in formalin. He said, however, that it was better to dry them and make mummies of them.

He seems to think that the formalin injures the ornaments and other finer structures. Perhaps, however, it will be safer to send them in both ways.

We would also prize much any of your bird malaria or other preparations. If the Italians can only see your results, it may stir them up to follow in your footsteps.

Naturally you would like to have your work confirmed by men who have done such good pioneering already. Of course you must guard yourself against sending us any specimens regarding which you have not published your results.

[1] Yet Grassi denied later (Chapter XXI) that I had ever found this mosquito.

It has been a cause of surprise to me how very closely they have followed all that you have done, and how fluently they talk regarding details of your work.[1] I need not assure you with what pride all Englishmen follow all you do. In Edinburgh Manson's quiet exposition of your latest results created quite a furore.

Though ill and suffering, he unfolded the story with great completeness and effect.

Yours very truly,

T. Edmonston Charles.

P.S.—I did not find Bignami at his laboratory, and called at his house after dark, with no better success. If I can get any details out of him likely to interest you I shall send them to you by next mail.

I received both these letters shortly after my return to Calcutta. Bignami had evidently infected a man just as I told Manson in my letter of 6 September Koch was likely to do. The announcement was received by me with mixed feelings: I had long wished to crown my work by infecting myself with an infected mosquito (quinine was always at hand to cure me); but on the other hand was now saved this disagreeable triumph and was glad to obtain such corroboration of my labours. With Dr. Edmonston Charles I was not acquainted; but as I knew the poor reputation of these Italian gentlemen in connection with the work of Laveran and Golgi, I confess that at first I thought him to be a busybody who was trying to get unpublished information from me for the advantage of questionable friends, and I was correspondingly reserved in my reply.

It will be remembered (page 194) that in 1896 Bignami had opposed Manson's induction but had at the same time tried to substitute another theory, that mosquitoes bring the *Plasmodia* from marshes and inject them into men by their bite [21]; and he added that some years previously he had tried to verify this theory by causing mosquitoes brought from a malarious locality (? larvæ or adults) to bite healthy men, but had failed. Since then we had heard no more of Italian work on the mosquito theory; but now that our papers of 18 December 1897, 26 February, 18 June 1898, and Manson's

[1] Yet Grassi stated later (Chapter XXI) that his labours were independent of mine.

address at Edinburgh on 28 July 1898, were known to all we expected further developments. Bignami says [38] that he began to resume his experiments (doubtless *indipendentemente da Ross*) early in August following—that is *possibly* before he had heard of Manson's announcement of the infection of birds by me. Anyway he proceeded chiefly by bringing *larvæ* from stagnant pools in the Campagna, hatching them out in Rome, and causing the *adults* to bite two men, G. N. and A. S., for many nights. Both cases remained quite negative of course; and he would probably have continued similar futile experiments indefinitely but for Manson's address on my work [34], which showed that infecting mosquitoes must be themselves infected by biting birds or men after being hatched from the larva—published in *The Lancet* of 30 August and *The British Medical Journal* of 24 September. His third experiment, therefore (which he says was commenced on 26 September), was done with *adult* mosquitoes, including *Culex penicillaris*, Grassi's "*Culex malariæ*," and *Anopheles claviger* (*maculipennis*), collected from Maccarese, a malarious place in the Campagna (he does not say how). The patient A. S. (Sola) began to have fever on 31 October, and *Plasmodium falciparum* was said to be found in him on 3 November. He had probably been infected by the *A. claviger*, which had previously bitten infected persons at Maccarese. The experiment was thus really only a repetition of my experiments on birds carried out three months previously. It was of little scientific value but Bignami claimed to have entirely proved the mosquito theory by it!

His paper [38] appeared on 15 November; and a translation of it called "The Inoculation Theory of Malarial Infection Account of a Successful Experiment with Mosquitoes," was published in *The Lancet* of 3 and 10 December 1898—reaching me about the end of the year. It was a wordy confused paper cleverly written so as to conceal the author's obligation to us. In order to avoid this charge of concealment he mentions and praises my work up to Manson's announcement [34] of July 1898; but also, in order to avoid the obligation, he gives no clear account of my work, nor even of my infection of healthy birds, nor does he explain the real reason why he changed his experimental procedure (from the use of mosquito larvæ to that of adults) in September for his third experiment—a vital and fundamental change. He leaves it to be inferred by the ignorant reader that his success was independent of my work and due to the same kind of experiments as those which he had

used in 1894—which was not the case. And of course he completely ignores the zoological rule of priority, that the life-cycle of *Plasmodium relicta* (*Proteosoma*) covers that of *P. falciparum*. Personally I would not care to write such a paper.

The Roman writers possessed many advantages over me. They were free to do as they pleased; they could publish their results a few days after obtaining them; there were numerous well-known intensely malarious spots close at hand; and there were many hospitals and laboratories and professors to consult if necessary. Above all, their mosquitoes were already classified and described in Ficalbi's monograph of the Italian Culicidæ [24], while I could obtain no information whatever on the subject in India. Lastly they had good libraries and all my experience and writings to guide them. To continue Charles's letters then:

72 Via di San Niccolo da Tolentino, Rome,
November 19th, 1898.

Dear Dr. Ross,

Since my letter, dated the 4th, they have had no more successes in conveying infection through the mosquito.

The fact is, the Vienna Plague outbreak has made it very difficult to persuade people to allow themselves to be bitten by mosquitoes.

As, doubtless, it would help you to have named specimens of the mosquitoes spoken of by Grassi, I went to his laboratory to try and get him to give you a few specimens of the different kinds of mosquito.

I did this under the impression that he had completed his investigations. He told me, however, they were far from complete, and did not give me the specimens.

He spoke in the highest terms of praise of your work; he has your first report,[1] and told me to write to try and get your future reports at an early date for him. I therefore write to India for them to-day.

Late last night Grassi sent me a note on malaria propagated by special insects. He presented it to the Royal Academy of the Lincei on the 6th of November.[2]

It had not been published the day before yesterday. As

[1] On the Cultivation of Proteosoma [32], containing full details of my technique, etc. (page 288); sent by Manson.

[2] See References [36].

the post goes this forenoon I have just run my eye over it, and send you the following extracts:

He thinks the grey mosquitoes you sent him are the Culex pipiens. The word mosquito is a vulgar term that includes many diverse, and, in part, many different genera of insects (Culex, Anopheles, Aedes, Ceratopogon, Simulia, Phlebotomos). The included species amount to many over a hundred.

Briefly, his previous researches may be arranged in a resume thus: (1) The Ceratopogon, the Simulia, the Aedes, and the Phlebotomos are, at the least, not necessary for malarial infection. (2) Certain species of mosquito, amongst which the most common, Culex pipiens, ought to be held innocuous. (3) Certain other species are at the least not necessary—Culex Richiardii, Culex annulatus, Culex hortensis, Anopheles bifurcatus, Anopheles nigripes, Culex spathipalpis, Culex pulchritarsis. (4) Certain species are under great suspicion; these are the following—Anopheles claviger, Fabr, Culex penicillaris, Culex malariæ. My servant,[1] who was infected with malaria, was only bitten by these three species in the month preceding infection. I was thus led to the conclusion that the first two, at least the second, possibly even the third, ordinarily furnished the alternating host of the malarial parasite of man.

Bignami's first ineffectual experiments seemed to have been conducted with the Culex pipiens, and perhaps, with the Culex hortensis.

It may be asked, are all the three species to blame, or two, or only one of them? I suspect, and also say that I believe it to be ascertained, that the Culex penicillaris, propagates malarial fever. I cannot, however, in fact deny that the other two forms do the same.

In order to infect, malarial mosquitoes must themselves be infected. The greater number of them are certainly not so.

All that we know of the cycles of evolution of animal parasites makes us affirm that marsh mosquitoes are the only way of the transmission of malaria.[2]

[1] Charles is here evidently quoting direct from Grassi, though there are no quotation marks in his letter.

[2] Quotation apparently ends. All these opinions on the species of mosquito concerned in malaria were mere guesses without scientific evidence, and were mostly wrong (page 407). Yet Grassi subsequently claimed to have found the "*Anopheles* malariferi" independently of me, not only for Italy, but for the whole world, *solely* on the strength of these bogus conjectures; and a number of incompetent writers still credit him with the achievement!

In a polemic on the subject, Grassi published a letter in the *Corriere della Sera*, of the 13th November, in which he boldly states that it is quite unnecessary to have resort to the hypothesis of malarial germs in the soil.

Grassi showed me in the blood of the owl two small parasites, very slightly pigmented, which I could not distinguish from the summer-autumn parasite of human malaria.

This, together with Dionisi's observation of quartan, and summer-autumn parasites in bats, opens out a huge vista for careful investigation.

One difficulty of accepting without reserve the mosquito theory was the fact that malaria seems to abound where there are no men. If bats, owls, and possibly other twilight feeders, furnish the appropriate intermediate hosts for malarial parasites, man may be considered as a mere accidental host.

Man suffers when the spores with their attendant toxins are discharged from the red blood corpuscule. Sporulation does not seem to have been noticed in bats or owls, hence they do not seem to suffer much from affording a resting-place for the parasite.

It would be very interesting if you could go a little into this enquiry. Probably, if man is an accidental host, and the night-feeding mammal or bird the really natural host of the parasite, it would seem to be extremely likely that you get the parasite to develop in the mosquito more easily from one of the night-feeding creatures than from man.

Anyhow, if you have the same difficulty in getting subjects to allow themselves to be bitten by artificially infected mosquitoes, bats, owls, etc., will prove a godsend to you.

I have been trying for some days to get the book-seller to procure for me a copy of Ficalbi, on the various species of the European Culex. He has not yet found it for me. If I can get a copy I will try and translate for you the description he gives of some of the species referred to by Grassi.

Grassi seems very friendly, and several times he assured me that if you sent him any specimens of mosquitoes he would name and classify them for you.

It seems there are many grey mosquitoes besides the Culex pipiens. One dappled-winged mosquito is the Anopheles claviger.[1]

[1] They had recognised the genus by the eggs *plus* the wing-marks (page 403).

Grassi would like dried specimens sent in a pill-box, with a little naphthaline. He particularly wants males as well as females. These resemble the female, but have ornaments on the proboscis which prevent them from biting.

Grassi would also like some specimens sent in alcohol. If Grassi can name us a certain number of mosquitoes for you, it will enable you to follow future Italian work with more exactness, as they will doubtless all name the mosquito with which they are working.

Yours very truly,

T. EDMONSTON CHARLES.

72 VIA DI SAN NICCOLO DA TOLENTINO, ROME,
November 25th, 1898.

DEAR MR. ROSS,

I went to Professor Grassi's laboratory and showed him one of your preparations (third day, from the sparrow), showing pigments, etc., in the walls of the mosquito's stomach. He seemed much pleased, and I understood him to say he had just found it in a preparation of his own,[1] and that in a week he hoped to have verified all your work.

He had before him *The British Medical Journal*, with your paper of the 18th December, 1897, and seemed perfectly satisfied that your description of the mosquito referred to the Anopheles claviger.[2] He had numbers of these mosquitoes in a bottle, and volunteered to give me one to send to you. He had no males, but I send you two females by to-day's post. This mosquito, however, seems darker in colour than the one you describe, although you use the word "dark brown" species to designate it.

I note that lower down you say that the back of the thorax and the abdomen is a light fawn colour.

In a previous letter I told you that at the Santo Spirito they looked upon the Anopheles claviger as your dapple-wing, which, of course, it is not.[3] On my mentioning to Grassi that they had not succeeded at the S. Spirito at finding any

[1] This statement of Grassi's does not agree with the opening sentence of Charles's next letter. What a pity that the former did not show his specimen to the latter at the same time!

[2] Which carries malaria in Italy. Yet afterwards Grassi said he found it independently of me. *A. claviger* has nearly the same eggs and wing-marks as my large dapple-wing, in which I first cultivated human malaria fifteen months previously (page 233). Evidently Charles also was familiar with my paper [29].

[3] Not the same species, but the same genus.

development in the Anopheles claviger after feeding it, both on spring tertian blood and on blood containing summer-autumn parasite (the blood was apparently favourable, as I found crescents in the one fever case, and large flagella-producing forms in the spring tertian), Grassi threw out the suggestion that, as the mosquitoes had bitten in a cold ward, the sudden change from the warm blood of the man to the cold stomach of the mosquito may have been too much for the parasite, and inhibited the changes wished for. Grassi breeds his mosquitoes in a small room with a stove; the temperature felt to me over 80° Fahr.

At the time of the experiment in the S. Spirito I should judge the temperature to be just over 60° Fahr., having as a guide the temperature in my own house at that time.

In your future reports it would help to throw light on this point if you stated the temperature at which you worked. If in very cold weather you can procure a case of malaria, you might heat up a small room by lamps, or otherwise, to put your fever patient in during one of your experiments.

Bignami has written a paper, not yet published, which Dr. Brock is translating for *The Lancet*. I have asked him to try and get me some reprints of it, as it is so much more handy for reference in this form.

If he succeeds they ought to be here in about a fortnight, and I will send you a copy.

Bignami's one success in producing fever through the bites of mosquitoes will be described in it and a good many speculations and clever arguments are sure to be gathered together. Besides this, the finding of chromatin in the flagella, and the probable import of this discovery, will be dwelt on.

Yours very truly,

T. EDMONSTON CHARLES.

P.S. I send you a translation of Grassi's first paper.

72, VIA DI SAN NICCOLO DA TOLENTINO, ROME,
December 2nd, 1898.

DEAR DR. ROSS,

Within four or five hours of posting your letter last week they found your pigmented bodies in the walls of the stomach of a mosquito.[1] It had been fed on crescents.

[1] *A. claviger*, which they thought was my "dapple-wing"!

On the 28th November their success was reported to the Royal Academy of the Lincei, as appears from a reprint, which I send you by to-day's book post from the authors.

I send you, also, a translation of this communication. You will see they report success up to the fourth day. Since then they have only advanced to the sixth day, and, consequently, have not met with your germinal rods.

For some days they have had a great mortality amongst their mosquitoes; but they tell me to-day that they have succeeded better in keeping them alive.

I fancy this must be due to their following your tactics more closely as regards feeding, water, and daily change of tubes.[1]

Bignami gave me a copy of his new paper yesterday; there are thirty-one pages of it. I have just had time to run my eye over it. There is nothing of much importance that you should know at once. In fact, I have referred to almost every point in my previous letters. He defines very clearly his position with regard to the flagella, and seems ready, as soon as proof can be secured by experiment, to throw over both air and water as media of communication of malaria.

If you have plenty of material you would help much in settling the question by making people breathe dry germinal rods and others drink them. This question is so important, and, in a sense, constitutes a part of your enquiry, that I do not hesitate to urge it on you.[2]

The season is too far advanced, and the material too scanty, for the Italians to achieve much more success.

I have told them how pleased you will be to hear that such competent specialists have been able to confirm your work.

Perhaps with their report in your hand it might suit you to mention the fact in your next communication to the Government.

I fear we shall not be able to get your third report till well on in February, and that is a long time to wait.

If you can spare ten minutes, do tell us in half a dozen lines what progress you have made, especially whether you have been able to trace the germinal rods in man's malaria, and whether you have succeeded in producing fever through bites of artificially infected mosquitoes.

[1] These clever gentlemen never mention such trifles in their papers.

[2] Like advising a cobbler to make boots.

You know already that they have failed in cultivating quartan fever in the Anopheles claviger. Apparently their previous failure with crescents may have depended upon the crescents being immature, as they had only just begun to form in the case experimented with.

If you chance to be anywhere with quartans and tertians abounding, I fancy you will not have much difficulty in recognising what mosquito you ought to work with, as the species causing the mischief should also be abundant.

I do not know whether I should allude to the fact, that, in their communication of the 28th November, a suspicion of doubt is hinted at as to whether, possibly, your two mosquitoes before they fed on man had not previously bitten some other animal.[1]

From a purely scientific point of view I suppose it is impossible to disprove this suggestion; at any rate, it can display no animus against you, as all three writers acknowledge unreservedly the vast importance of your work, and the great successes that you have achieved.

Grassi, in fact, every time he sees me, again and again is jubilant in his praises.[2] He does not seem to be making any progress in confirming what you have published about the proteosoma. He cannot get the Culex pipiens to bite, and thinks that they are too young as yet.

Under mosquito nets they have large numbers of the Anopheles claviger. They seem to be more successful in keeping them alive in this state of comparative freedom than imprisoned in test tubes.

So there seems to be a possibility of procuring enough of material to continue their studies for some time longer.

I shall send you some more translation if ready in time for to-day's mail, otherwise by next week's mail.

Yours very truly,

T. Edmonston Charles.

The success reported at the beginning of this last letter was obtained with *Anopheles claviger* (Fabr.), also called *An.*

[1] Yet Charles and his friends must have seen the second sentence in my paper [29] which they had all read and *studied carefully* and in which I stated that my mosquitoes were "bred in bottles from the larva" (see page 350).

[2] Yet his researches were independent of mine.

maculipennis, Meig., and *An. quadrimaculatus*, say the common *Anopheles* of Europe and other countries. The Italians had already recognised my "dapple-winged mosquitoes" to belong to this genus from the description of the *four spots* on the wings and the peculiar boat-shaped eggs given in my paper of 18 December 1897, in which I first described finding the pigmented cells in a species of this group (page 233). The Italian success was therefore merely a confirmation of my original discovery of the human *Plasmodia* in *Anopheles* made on 20 August 1897, fifteen months previously. Any honest men of science would, of course, have said so in the very beginning of their paper; but Messrs. Bastianelli, Bignami, and Grassi reported the matter a few days later [40] as follows (I add Dr. Charles's translation):

REALE ACCADEMIA DEI LINCEI

Zoologia Medica.—*Coltivazione delle semilune malariche dell' uomo nell' A n o p h e l e s c l a v i g e r Fabr. (sinonimo: A n o p h e l e s m a c u l i p e n n i s Meig.)*. (1). Nota preliminare di G. Bastianelli, A. Bignami e B. Grassi, presentata dal Socio B. Grassi.

Ci siamo proposti di studiare i rapporti delle singole zanzare colle singole specie di parassiti malarici dell'uomo e, avuto riguardo alla complessità dell' assunto, abbiamo trovato opportuno di lavorare assieme.

Come primo risultato delle nostre ricerche possiamo annunciare che abbiamo potuto seguire con tutta sicurezza parecchie fasi di sviluppo dei corpi semilunari nello spessore dell' intestino medio di parecchi *Anopheles claviger*, tenuti a temperatura opportuna, ai quali avevamo fatto succhiare sangue di individui affetti di forme malariche estivo-autunnali. Le notevolissime fasi in discorso trovano riscontro in quelle descritte dal Ross (2° e 3° giorno) per il proteosoma (emameba) degli uccelli.

In una camera dove degevano quattro malarici (affetti tutti di febbri estivo-autunnali ?) abbiamo raccolto:

sei *Culex pipiens*:
un *Anopheles nigripes*:
quattro *Anopheles claviger*.

L'esame di questi Culicidi risultò negativo, tranne per due *Anopheles claviger*, nei quali trovammo stadî di sviluppo

(1) *Inviata il* 28 *Novembre* 1898.

ulteriore, corrispondenti a quelli descritti dal Ross per il proteosoma degli uccelli (4° giorno).

Verisimilmente i due *mosquitos* colle ali *macchiate* (1) nei quali il Ross in India trovò stadî di sviluppo simili a quelli del proteosoma (3° giorno circa) appartenevano pure alla specie *Anopheles claviger Fabr.* Il Ross però non avendo seguìto lo sviluppe di questi corpi, non poteva con sicurezza riferirli alle semilune, essendo anche possibile che i suoi due *mosquitos* prima di pungere l'uomo avessero già punto altro animale.

Negativi finora riuscirono molti altri tentativi di coltivare nell' *Anopheles claviger* i parassiti della civetta e dei piccioni.

Si noti infine che a Lentini (Sicilia) nell'ottobre e nel novembre scorso, pur infierendo la malaria, non si trovò nè *Culex penicillaris*, nè *Cilex malariæ ;* gli *Anopheles claviger* invece erano straordinariamente abbondanti.

(1) Esistono in Europa cinque culicidi colle ali macchiate. Per l'India non conosciamo alcun dato.

Translation

Royal Academy of the Lincei

Medical Zoology.—Cultivation of the malarial crescents of man in the Anopheles claviger Fabr. (Synonym Anopheles maculipennis Meig.) Preliminary Note by G. Bastianelli, A. Bignami, and B. Grassi, presented by the Associate B. Grassi.

We have proposed to study the relationship between the special species of mosquitoes and the special species of parasites in the malaria of man, and having regard to the complexity of the undertaking we have judged it opportune to work together. As the first result of our researches we can announce that we have been able to follow with all certainty, certain phases of the development of the semi-lunar bodies in the thickness of the middle intestine of several (other [2]) Anopheles claviger, kept at a favourable temperature, which we have made to suck the blood of individuals affected with the Summer-Autumn form of malaria.

The most notable phases in question find a comparison in those discovered by Ross (second and third day), for the proteosoma (hemamæba) of birds.

In a room containing four malarial patients (all affected with

[2] Not in the original. R. R.

estivo-autumnal fever ?) we have collected : six Culex pipiens, one Anopheles nigripes, four Anopheles claviger.

The examination of the Culicidæ led to a negative result except in the cases of two of the Anopheles claviger, in which we found the further stages of development corresponding to those described by Ross for the proteosoma of birds (fourth day). Apparently the two mosquitoes with spotted wings (note : there exist in Europe five species of Culex with spotted wings. In India we do not know of any data) in which Ross found in India the stages of development similar to those of the proteosoma (about the 3rd day) belonged also to the species *Anopheles claviger* Fabr.

Ross however, not having followed the development of these bodies could not with certainty refer them to the crescents, it being also possible that his two mosquitoes, before having bitten men had already bitten another animal.

Up till now many other attempts to cultivate the parasites of owls and pigeons have given negative results in the Anopheles claviger.

We have to note in conclusion that at Lentini (Sicily) during the months of October and November Malaria prevailed much, but the Culex pencillaris and the Culex malariæ were not to be found ; the Anopheles claviger on the other hand was extraordinarily abundant.

November 28, 1898.

It will be observed :

(1) That these people do not mention where exactly my work was published, so that their hearers or readers could not verify their statements regarding it.

(2) Their statement that, " essendo anche possibile che suoi due *mosquitos* prima di pungere l'uomo avessero già punto altro animale " was directly contrary to the second sentence in my paper in *The British Medical Journal* [29], 18 December 1897, in which I said : " For the last two years I have been endeavouring to cultivate the parasite of malaria in the mosquito. The method adopted has been to feed mosquitos BRED IN BOTTLES FROM THE LARVA, on patients having crescents in the blood, and then to examine their tissues for parasites similar to the hæmamœba in man." The rest of my paper reported the finding of such parasites ; and the whol

case was fully proved by my subsequent work on human and avian parasites.

(3) Messrs. Bastianelli, Bignami, and Grassi must have had this paper of mine before them when they wrote their note (see Charles, page 344). Their statement was therefore a deliberate and intentional lie, told in order to discredit my work and so to obtain the priority for themselves, and constantly reiterated even after I had exposed it.

(4) It is amusing to observe that they themselves say nothing about their own mosquitoes having been bred from the larva. Also they had succeeded only with two insects! Yet they demand the "tutta sicurezza" for their observations which they refuse for mine.

(5) The falsification is so obvious that one may doubt the truth of the whole Nota Preliminare given above (see also Koch's letter, page 408). It is quite *possible* that these gentlemen were at that time merely showing my preparations which Manson had sent them.

Their next paper [41] was dated 22 December 1898, and was published by the same Academy. It describes the finding of "pigmented cells" in some more *Anopheles claviger*, but does not state exactly in how many or whether the insects used had been bred from the larvæ. In fact, the article might have been transcribed bodily from my *Proteosoma* Report and Manson's Edinburgh address. My name is mentioned only once, on the fifth page, where the three authors say: "Le osservazioni fin qui riportale permettono di ricostruire il ciclo di vita degli Emosporidi umani nel corpa dell' *Anopheles claviger*: esso trova in gran parte riscontro in quello osservato da Ross per il *proteosoma* degli uccelli nel gras mosquito"—which Charles translated: "The observations reported up till now permit us to reconstruct the life-cycle of the human hæmosporidia in the body of the *Anopheles claviger*: it finds for the most part confirmation in that observed by Ross regarding the proteosoma of birds in the *grey mosquito*." In other words, my work confirmed that of these rascals! Again, no exact references to my writings are given.

From the intrinsic evidence of these two papers, I believe with Koch (page 408) that it is quite *possible* that the alleged observations were never really made at all, but that the papers were written entirely from my specimens and descriptions in order to obtain priority for the discovery of the human parasites in *Anopheles*—which the writers then thought I was making in India. More *probably*, however, they did find the pigmented

cells, but only in some old *Anopheles claviger* which had been *caught in rooms* where malaria cases were sleeping—as, in fact they actually say in their second *Nota preliminare*; that is just as I found the cells some months later in *Anopheles costalis* caught in Wilberforce Barracks in Sierra Leone. Some of these old *A. claviger* may have been refed subsequently on the malaria cases in hospital; but in neither *Nota preliminare* is there any statement whatever that the insects employed had been bred from the larvæ in captivity and then fed on the malaria cases in hospital for the first time—no such statement in fact, as I had clearly made in my article of 18 December 1897. Thus the very ambiguity which the authors falsely asserted against my work existed in their own papers when they made the assertion. Of course the Plasmodial nature of the pigmented cells in their mosquitoes was guaranteed "con tutta sicurezza," not by any work of theirs, but by my preceding proofs of the whole-life cycle of *Proteosoma* in mosquitoes. Can any more impudent scientific frauds than these two Preliminary Notes of Messrs. Grassi, Bignami, and Bastianelli be imagined?

This sort of thing is not uncommon in science, which it impedes and disgraces. All cases of it ought to be publicly exposed when detected; and I therefore do not mince my words regarding the present one.

Dr. Charles wrote me four more letters from Rome, up to 14 January 1899—they need not be given here. When I received his three first letters I did not appreciate exactly what had happened, and consequently wrote him a very gentle corrective reply on 19 December; but when I received the fourth one (of 2 December) the piracy became manifest, and I criticised the Italian work somewhat shortly and closed the correspondence. Similarly I thought well of Grassi at first and in my paper for the Institut Pasteur (which was finished on 31 December 1898) actually said that he was working independently of us! I had not seen all his papers then: and, indeed, he did not fully develop his ingenious strategy till he wrote his book, published in June 1900—which I will deal with later (page 401). I soon heard, however, that the brigands had fallen out over their booty; but I now leave them in full enjoyment of it.

On 13 December I sent Charles some specimens of *Proteosoma*, some grey mosquitoes, and a dapple-winged one. On 19 November he wrote two surprising official letters, to the India Office and to the Government of India respectively, asking for immediate copies of my reports and for specimens from me,

and saying how familiar the Italians were with my work, and so on. These letters reached me (as official prints) in February 1899, and I was commanded to do as Dr. Charles wished. My friends blamed Charles for writing them—I am sure that he acted in good faith, but was used as a cat's-paw by his clever acquaintances.

Hitherto I had always passionately denied the truth of Tennyson's lines :

> The man of science himself is fonder of glory, and vain,
> An eye well-practised in nature, a spirit bounded and poor.

But now I felt like one who has long been living with distinguished and agreeable friends whom he suddenly finds to be a gang of sordid aventurers. Science was never the same again to me.

Of course Manson knew all about these doings. On 7 December he wrote : " You will see from *The Lancet* the position Bignami is taking and how there is an attempt to belittle you while exalting himself. All this is very amusing and contemptible. . . . You have plenty of friends here who know how to uphold you and your rights. Your work is the starting point of all this mosquito business." On 10 January he wrote very kindly : " You have forged the key and others must take the trouble to open the door . . . we will take care here that your position in this matter is not ignored. . . . That kala-azar business was most unfortunate. Had you been left alone you would have anticipated all that the Italians are doing. Practically you had already done it and you can justly claim to have been the first to find the malaria parasite in the mosquito as well as to point out the modus operandi of that insect in diffusing malaria. . . . Two mails ago I wrote to Harvey telling him that I hoped he would get the Indian Government to make some recognition of the splendid work you had done. I pointed out that its enormous importance was already recognised in Europe, and that honours were in store for you here. I told him that it would be a graceful act for the Indian Government to anticipate these. I also wrote in the same strain to Colonel Fenn, whom I know, to influence the Viceroy in the same direction." On 18 January he wrote : " The Romans are still active. I wrote last week to Bignami telling him we were careful of your reputation and hoping that he and his compatriots would be equally so. They are all on the grab it would seem. I had a letter from Charles today saying that there are now some twenty or thereby claimants for the mosquito theory. I daresay in another month there will be fifty. . . .

But don't let your wish for completeness keep you a day longer in India than your health will stand. Don't risk anything. Your work is practically complete and you should come home for a time. . . . They are getting up a tropical school in Liverpool, and came to me about a lecturer, etc. Amongst others I suggested your name, and Boyce has jumped at the idea."

These were the last letters that Manson wrote to me in India—a noble series such as few men have received.

I do not think that when he wrote to Bignami (of course, without effect) he could have read with attention the later Italian papers. My complaint was not that the Italians had anticipated me regarding the completion of the human malaria work—a trifling matter—but that they had sullied science by misrepresenting the facts. In spite of Manson's statements above, I can remember little exposure of this in the scientific press at home. I still possess the draft of a letter to *The British Medical Journal* in which I mildly attempted to expose the Italian doings. It was sent in before 5 January 1899 (my letter to Manson of that date); but for some reason was never published. No English periodical ever published my remonstrances, and none of my English friends ever fought for me. I was obliged at last to carry the fight into Italy (page 411). *The Indian Medical Gazette* of March 1899, however, protested strongly against the piracy. I do not know for certain who wrote the article; but am much indebted to that journal for its previous publication of my work, and for the support then rendered. The March number also contains Charles's translation of the second *Nota preliminare* (page 351), which I had sent to Maynard, who had been an editor. Of course the victim of a piracy remains almost helpless, himself.

Meanwhile several people in India were in opposition, and my old adversary, Surgeon-Colonel Lawrie of Hyderabad, who was now converted to Laveranity, delivered a furious attack in *The Lancet* of 3 December (the number which printed Bignami's article), and also in the Indian papers under the name of Buggo butty Bose, M.D. Both articles were absurd, but they perverted many people, including even my friend, Mr. Reid of Nowgong. I was pummelled on one side by the pirates, and on the other by the sceptics, but was too exhausted to defend myself from either.[1]

I had seen my chief, Surgeon-General Harvey, Director

[1] I was asked later which I preferred, the thieves who stole my pearls, or the swine which trod them into the mire. I replied that, at least, the thieves knew their value. It requires some mental acuity to be a pirate, none to be a "pseudo-sceptic."

General I.M.S., when he was taking over the office from Surgeon-General Cleghorn, on 17 February 1898 in Calcutta. Since then he and his Secretary, Surgeon-Major Leslie, had been at Simla (I think that I never met the latter). They congratulated me politely when I reported my successes ; but they asked no questions, never sent for me, never ordered anyone to inspect or to verify my work, never gave me any assistance (except that of a native draughtsman who was of no use), interrupted me for five months over kala-azar just at the climax of my labours, and refused to assure me that I was to be retained on malaria duty after the 17 February 1899. Owing to constant changes of station and the necessity of living in separate hotels, we had spent more than £300 (about 4,800 rupees) since 1895 over and above my salary [1] ; and if I were now sent back to my regiment at Secunderabad I should have to pay the expenses of my family's journey there, and next year should be obliged to go with the regiment to Burma. On 6 December I wrote to Leslie explaining all this, asking for six months' furlough to England at the end of my special duty in order to place my family there if I was to return to my regiment, and begging again for the required assurance. In reply he only told me that Dr. Harvey was coming to Calcutta on the 15th. I was granted by the latter a three-minutes' interview, in which he complimented me upon my work, but refused to come to my laboratory. He allowed my leave to England, but could not promise to continue me in the malaria work ; and struck me as being a mild but ineffectual man who took little interest in the matter. Soon, however, the confirmation of Drs. Wright and Ruffer and of the Italians must have reached him (I sent him one of the latters' Notes), and he wrote :

DIRECTOR GENERAL INDIAN MEDICAL SERVICE, CALCUTTA,
2.1.99.

MY DEAR ROSS,

I have spoken to the Govt. and to the Hon. Member in the Home Dept. and do not think they will make any difficulty about putting you again on Special duty when you return from leave. I think therefore you may safely apply for furlough as I know you need a rest after your hard work.

Yours sincerely,
B. HARVEY.

[1] This was 1,000 rupees a month while I was on special duty and less on regimental duty. A friend, nine years my junior, who was one of the Calcutta medical officers, was drawing 1,700 rupees a month, and had a lucrative private practice as well.

In other words, this chief of a great service entrusted with the health of three hundred millions of people was not free to continue one of his own officers in the investigation of a disease which killed over a million of those people annually—without going round begging to laymen for permission.

Of course this letter was useless to me—Members of Council were apt to change their mind. In order to carry out the second part of my programme, the study of the local carriers of malaria in India, I should require at least five years' permanent duty there, with an adequate salary and staff. It was absurd to expect such things then in India. I had to deal with an Army without an Intelligence Department, and with a general who took no interest in the enemy's movements and did not trouble to ascertain what I required. I therefore bought my passage home at once, and decided to retire on reaching England.

Interleaved in my laboratory note-book there is a cutting from *The Pioneer* describing the dinner of the Indian Medical Service held at Simla on 19 September 1898.[1] Harvey presided and proposed "prosperity to the grand old service to which we are all proud to belong . . . we shall go on doing our duty in the future as in the past (applause). . . . I claim for the Indian Medical Service that it rises to its duty and is true to itself on all occasions (loud applause). . . . I saw it stated by a correspondent in *The Pioneer* the other day—I wish people would not write to the papers—that the I.M.S. is a decadent service. Don't you believe it, gentlemen (applause) "—and so on. If I had been present I should have applauded as loudly as anyone; but the speech was that of the colonel of a dragoon regiment rather than that of the head of a great scientific corps. There was not a word about science in it. The service had done all he claimed for it; its defect was that it had not kept pace with scientific advances in recent years. The waving of swords will not win victories; and the waving of lancets and prescriptions will not maintain empires in health. Nevertheless, I thought more highly of Dr. Harvey when, some time later, he came to me in Liverpool and frankly admitted the mistakes which had been made (page 390).

The truth is, that of the few medical problems which have been solved most have been solved by accident or by some lucky lead in other sciences, few indeed by express intention and long effort; and when such a case occurred it met with general incredulity among the many who never "read, mark,

[1] On the same date we were given full military titles without the prefix "Surgeon," which many disliked.

learn, and inwardly digest." One can scarcely blame these people more than one can blame the venerable cow which impedes our motor car in the narrow country lane! She was born in the quiet fields; to her the noisy engine of progress is a thing either to be fought or to be feared; she opposes it with horns or heels, or scampers foolishly before it—until, escaping up some lucky by-lane, she can watch it with bovine anger, safely receding in the distance, and so go back to her peaceful browsing!

Dr. Laveran had long asked me for an account of my work for the Paris Académie de Médecine. My article [42] was finished on the last day of 1898,[1] and a translation of it was presented to the Académie on 24 January 1899, and was reported upon by Laveran on 31 January [43]. Dr. Laveran's report (which reached me later, page 365) gives me full credit; and my paper repeated the acknowledgments to him and to Manson which I gave in my Proteosoma Report (page 287). Let me hope then that, at least, I shall never be included in the great genus *Savant parasite!*

Lastly, on 16 February 1899, just before leaving India, I sent to the Director-General a concluding report "on the subject of the practical results as regards the prevention of the disease (malaria) which may be expected to arise from my researches." This described briefly the methods which were subsequently employed with success in West Africa, Ismailia, Havanna, the Federated Malay States, Panama, and elsewhere. The article [45] was published in *The Indian Medical Gazette* for July 1899, and will be dealt with in the third Part of this book.

In those days, when an officer had been placed on special duty for any purpose and his work was done, orders conveying the thanks of the Government which had employed him were generally issued as some record that his duty had been faithfully performed. I was not so fortunate; and from that day to this have never received any recognition that I am aware of from the Government of India, or even from my old service. I have never been consulted even on my own subject by that Government or by the India Office; never placed on any committee connected with it; never been asked officially for my advice; never received any Indian honour, honorary promotion, or reward. In 1912 I was advised to ask for one of the small "good service pensions" which were then sometimes

[1] This was before I changed my opinon regarding the "black spores" (page 328).

given; but science evidently did not count as such, and I was refused. Out of nearly fifty honours bestowed upon me by governments, universities, academies, and societies, only one[1] has ever reached me from India; Indian writers, from G. M. Giles downwards, have generally attributed my work to others; and, much worse than this, have obstinately opposed my methods for the reduction of malaria. In fact, they have never forgiven me—for what—I cannot imagine. Probably it was nothing but the sheer ineptitude of unintellectuals who "despise science"; but, if so, the question remains whether such barbarians are fit to rule so great an empire.

My letters to Manson during this period contain chiefly brief notes about our failures; but I am glad to record, in honour of the memory of Mahomed Bux, the following from my letter of 5 January: "Mahomed Bux however, who can make a mosquito do anything, persuades them to bite in the day-time simply by holding the test-tube (containing the insect) against the skin of the patient." I forget who discovered this a year or two later. Let me now conclude with extracts from my two last letters of this series.

CALCUTTA,
15 *Feb.* 1899.

DEAR DR. MANSON,

Daniels went away on the 12th for Africa. Our bad luck has clung to us to the last. Anopheles pictus [really *A. rossi*] has absolutely refused crescents, and also tertian—according to a few experiments. I cannot get a single case of human malaria. . . .

I am feeling better since my report has been finished and often feel inclined to stay. I am leaving hosts of beautiful experiments to the Italians and others. It is a great pity, but can't be helped.

We made an observation lately which throws doubt on the authenticity of the black spores—at least I think so—Daniels doesn't. I have reported to Government accordingly.

I have also reported to Government on the practical benefits likely to arise from the mosquito theory, if they follow it up properly. Will show you a copy of the report. . . .

. . . It would certainly be a good thing to do research in

[1] The Barclay Memorial Bronze Medal, Asiatic Society, Bengal, 20 May 1903. I was made Consulting Member, Advisory Board, Indian Research Fund but think that I have only once been consulted by it.

India steadily ; but there are other things to be considered and many cogent reasons why I should now leave India for good. I fear also that I may even sell my microscopes. My original intention was to take my pension and adopt literature—which I know will pay me better than the microscope, though I confess that it may not be so grand a thing as research on medical subjects. Now that the great problem is solved I feel I have done my duty and may sing *Nunc dimittis*, and look after those essential parts of clothing, the pockets. I don't think a man can be called upon justly to give up too much for the chance of doing good research in the future.

But I think Daniels will now push on matters nicely.

It is evident that the study of malaria has now entered on the third and last phase—prevention. That is, we must find out how to exterminate the malaria-bearing species of mosquito. I shall have a great deal to tell you on this score. . . .

We start on the 22nd and hope to arrive on about the 20th March—all the way by sea.

With kind regards,

Yours sincerely,

R. Ross.

"City of Corinth,"
24 *Feb.* *In the Hoogly.*

Dear Dr. Manson,

This is the last of a long series of letters to you. They began nearly four years ago.

I shall have your replies bound in a volume ! The solution of the great malaria problem is history you know !

We have been two days getting out of this river, the ship being a large one ; but are off at midnight. We hope to arrive on about the 22nd March. . . .

Well, I have left many things undone which I ought to have done ; but congratulate myself that the task you imposed on me is finished. You must cease *rubbing the lamp* for a little, at all events.

I am looking forward immensely to seeing you ; and I have a lot more to tell you than I have conveyed by letter. I shall concoct a full history [1] of the solution of the problem ; after

[1] This book is the "history."

the publication of which things will take their right places. It was the *great induction* which did it—nothing else.

With kind regards,

Yours very sincerely,

RONALD ROSS.

So I said good-bye to my laboratory—the mean little building in its grove of trees; to my birds and my mosquitoes; to my faithful Mahomed Bux, and my other helpers. Years later I heard that, as I wished, Mahomed had been retained by some one (? the Calcutta Municipality) in employment here until he died. Whether the laboratory still exists I do not know—no one has troubled to tell me. Ah, the beautiful researches which might have been! I have them still in my mind. They have never been done. By the aid of *Proteosoma* I had intended to work out the whole mathematical theory of parasitic infection, like Fourier's *Théorie Analytique de la Chaleur* and then to have investigated *treatment*, not medically, but scientifically. Had this been done, what sums would have been saved during the war! I could go back there to-morrow (if the place still exists) and start the work again exactly as I left it twenty-three years ago—not biology, but real measured science. But I was fated, I think, never to do any microscopical investigation again. Of course the great report on my work in July and August could never be written.

The enthusiast is rare, the successful enthusiast very rare. When he is a medical investigator he is perhaps the most valuable thing which the death-bereaved World can possess. But she does not know this, and throws him away like an old shoe upon the rubbish-heap. So they remain; he with his memories, and she with her dead.

On the quay we said good-bye to our faithful servants, Lutchman and Berlisi. We never heard of them again, though I advertised for the former in 1911.

Once more, as in 1865, I remember the brown water of the great Hugli turning green and then blue as the *City of Corinth* panted out to the ocean. The four years were past, but the Great Problem had been solved. Now it remained only to apply the solution—to apply it *immediately*—to the saving of human life on the large scale.

PART III

THE FIGHT FOR LIFE

CHAPTER XIX

THE NEW SCHOOL. LIVERPOOL, 1899

In the previous Part of this book I tried to give the reader an exact history of a real medical investigation, and I propose to devote a large portion of the present Part to a similar account of the manner in which the world received and acted upon the results of that investigation. Both records will probably be instructive and useful to those who desire the progress of the race; and this is why I am taking the trouble to write them now.

First, what exactly had the investigation given us? Up to 1898 the world thought that malaria was not communicable from man to man but was due to a poison (or germ) generated and given off by the soil of malarious areas, especially by wet soil, or marshes—but the world also knew, from the time of the ancients, how to reduce malaria by draining the soil. Now, however, my work of August 1898 had shown that malaria *is* carried from man to man, but that, nevertheless, it is also connected with the soil. The ancients were right after all. It is not the germ of malaria which is given off by the soil; it is the carrier of the germ, the mosquito. Now, therefore, instead of being obliged to drain the whole area of a malarious place, we need merely confine our attention to the spots where the carriers breed—a great improvement on the old method.

During our voyage home I had more leisure to think out the details properly. For some time I had conjectured that mosquitoes might infect, not only men, but each other by means of the "black spores"; but I now doubted whether these bodies were really any stage of the *Plasmodia* (page 328). The broader question remained, was there any other route of infection besides the bite of the mosquito? Both Laveran and Manson still thought so, but I now began to give up the idea. The known cycle of the *Plasmodia* was already complete, and already complicated: if they can enter us by another route, as, for instance, by the air or by drinking water, they must possess yet another cycle. This was inconsistent with

our general knowledge of parasites—of which thousands of species are known. If the *Plasmodia* can live in soil, water, or air as well as in men and mosquitoes, they must have special organs or appliances for each mode of life and must be extraordinary animals indeed, belonging to a class by themselves apart from the known remainder of creation [91, chapter v]. I suspected therefore that they are always merely passed from men to mosquitoes and back again; and this reasoning appealed to me with increasing force as time went on.

Another important theorem occurred to me about the same time. The question which species of mosquitoes do or do not carry human malaria might prove to be a very complex one, in view of the fact that hundreds of species are known. I therefore sought for some good argument which might help to limit the necessary inquiries. Malaria is connected with water on the ground—not with water in tubs, pots, and tanks close to habitations, but with marshes and pools; it is increased by rain and reduced by drainage. Hence it was extremely probable that *the insects which carry malaria breed generally or chiefly in terrestrial water*. My long negative work had almost proved that the common Indian "grey" and "brindled" mosquitoes do not carry malaria; and they breed mostly in pots, tubs, broken bottles, drains, etc. But the "dapple-winged" mosquitoes—of which two species had been found to carry malaria at Secunderabad, and one species, in Italy—bred almost entirely in natural collections of water on the ground.

What an instrument for good was now placed in our hands! We could go to a malarious town or village, find the culpable mosquitoes there by the method, discover and map their breeding pools, and then get rid of them by half a dozen obvious expedients. In more water-logged places we could always employ mosquito-nets with greater care than usual. I had undertaken my work for sanitary and not zoological reasons; and this was my reward. Revived by the cool weather in the Mediterranean, I remember my elation at these thoughts as we steamed slowly past the great and magnificent Sierra Nevada of the South of Spain. "In two years," I said to myself, "we shall stamp malaria out of every city and large town in the tropics—at least if they possess sanitary departments as in British possessions." And this was not the dream of a visionary. My experience of sanitation at Bangalore (Chapter XI) had taught me, what few medical men possess, a thorough knowledge of practical town-management, and I knew what I was talking about—sanitary organisation, town-

cleansing, sanitary engineering, houses, yards, drains, sewers, official procedure, and the rest of it. The thing could be done —almost everywhere in towns and villages ; wherever there was a white man or trained inspector to command and a few coolies to work : a little work every day, a few extra pounds on the estimates, a larger sum and a trained engineer for more difficult places, some good town ordinances and a man of sense to co-ordinate measures and to keep statistics. In fact I had already reported in this sense to the Indian Government [45] (page 357). Now, however, I was going to London—the centre of the Empire—and could appeal directly to the heads of British administration, the India Office, the Colonial Office, the War Office, the Foreign Office. Under them there were numbers of administrators, governors, commissioners, and other well-paid and powerful " proconsuls " ; and they could set the whole machinery in motion by telegram within a few months. The local tasks would be very easy in many places ; the breeding-waters of the culpable mosquitoes were generally likely to be insignificant : small pools or puddles, easily manageable by coolies instructed by sanitary inspectors, or sometimes even in more difficult cases, by minor engineering work such as any municipality or even village council could manage. In a few more months, perhaps in a year, or in two years, the death-dealing pests would begin to come under control, would begin to diminish, even to disappear entirely in favourable spots ; and with them, slowly, the ubiquitous malady would fly from the face of civilisation—not here or there only, but almost throughout the British Empire—nay, further, in America, China, and Europe. Not disappear entirely of course (perhaps an impossible ideal), but be at least banished from the centres of civilisation.

As of old, I had become another man by the time we smelt the Atlantic off the coast of Portugal. We reached London about the 20 March 1899, and took rooms for a few weeks at 10 Talbot Road, Bayswater ; but after a short but strenuous period of four months I was in the saddle again—this time, for Sierra Leone.

I was much cheered on arrival by receiving Dr. Laveran's report to the Académie de Médecine [43] on my article [42], which was presented by him to the Académie a week previously. In his report he said :

" L'idée d'attribuer aux moustiques un rôle dans l'infection palustre, accueillie d'abord avec beaucoup de scepticisme, a fait son chemin ; elle est aujourd'hui acceptée par la plupart

des auteurs qui s'occupent de l'étude du paludisme. Manson, en Angleterre, Koch en Allemagne, Bignami et Grassi, en Italie, ont publié des travaux très intéressants sur la question, mais ce sont surtout les patientes recherches de Ronald Ross qui ont mis hors de doute le rôle des moustiques dans l'évolution si compliquee de l'hématozoaire du paludisme et d'un hématozoaire endoglobulaire des oiseaux, très voisin de celui du paludisme.

" Depuis plusieurs années, je suis en correspondance avec M. le Dr. R. Ross, qui a bien voulu me tenir au courant de ses recherches et m'envoyer des préparations histologiques montrant les transformations des hématozoaires dans les moustiques ; je puis donc témoigner de l'admirable persévérance dont notre savant confrère a fait preuve dans l'étude de cette difficile question et aussi de l'exactitude de ses observations.

Le travail que M. R. Ross a bien voulu me charger de présenter a l'Académie est un résumé très bien fait de ses recherches, dont le début remonte a 1895, et qui ont déjà fait l'objet de plusieurs publications. . . . En terminant, je félicite sincèrement M. le Dr. Ross de l'importante contribution qu'il a apportée à l'histoire de l'évolution du parasite du paludisme en dehors de l'organisme humain, et je demande que des remerciements soient adressés a ce savant confrère pour le très intéressant résumé de ses recherches qu'il a envoyé a l'Académie."

The thanks of the Académie de Médecine were duly communicated to me on 9 February (see also *The British Medical Journal*, 18 February 1899, page 425).

Of course I went at once to lay my offering of four years' work at Dr. Manson's feet ; but, strangely enough, though I had long looked forward to the occasion, I cannot remember a single moment of the interview. He must have told me about his scheme of schools of tropical medicine, and especially about the Liverpool proposals (page 354), and I was off to that city on the last day of the month.

He first mentioned the scheme to me in his letter of 7 December 1898. It was probably suggested to him largely by his experiences with me in 1894, and I suppose that the malaria work now set fire to the faggot which would otherwise have remained a dead one. Being Medical Adviser to the Colonial Office (page 247), he was able to put his scheme before Mr. Joseph Chamberlain, then Secretary of State of the Colonies. A centre for teaching the technique of blood examination for malaria, of search for other parasites, and c

mosquito-entomology was certainly urgently required. The country already possessed two schools where this was or should have been taught—that at Netley, for Army medical officers, and that at Haslar for the Navy; but these were not open to colonial medical officers and civilians. I do not know what happened; but think that they should now have been opened for this purpose, or that, at least, the Colonial Office should have established a proper state-school on similar lines for its own doctors and for civilians. But this would have meant that the colonies and the Colonial Office would have had to pay for their "medical benefit"—a disagreeable necessity; and Mr. Chamberlain had a better idea. He issued a circular inviting various medical schools and hospitals which frequently had cases of tropical disease under treatment to start courses of instruction in tropical medicine and pathology. The Seamen's Hospital at Greenwich and its branches (Dr. Manson worked on the staff of one of these) was such an institution, and the University College and Royal Southern Hospital at Liverpool formed another. Both responded to the idea, and Liverpool was ready to start at once. Of course someone would have to pay for the laboratories, lecture-rooms, and teaching staff. I believe that the Colonial Office hinted at some small subsidies, but really the money was to come from local private subscriptions. Now this is a very uncertain source of income, and the result has been that the teachers themselves had to do most of the paying by drawing quite inadequate salaries for years. It was a very clever scheme, which would probably have been impossible anywhere outside Great Britain: Mr. Chamberlain [1] obtained great credit for it.

Shortly after arrival in London I wrote to Dr. Rubert Boyce, Professor of Pathology at University College, Liverpool, to make enquiries, and on his invitation went there on 31 October 1899. He was full of enthusiasm for Mr. Chamberlain's idea. The citizens of Liverpool were subscribing largely to University College, and some of them promised to support him in this new venture—especially his friend, Mr. Alfred Jones, head of the

[1] How his son dealt with the result will be seen on page 516. My own scheme was different. After 1898 I half expected that our grateful countrymen would give Manson and me pecuniary rewards such as they give to successful generals and gave to Edward Jenner in 1802 and 1807 for vaccination (£30,000)! I proposed to use mine for founding an Imperial Malaria Institute for investigation and the centralisation of records—something like Tycho Brahé's Uraniborg; and wished particularly to study treatment and prevention in armies during war. If John Bull had been wise and honest enough to do this he would have saved himself millions of pounds for the Salonika malaria alone (page 519). But I counted my chickens before they were hatched.

large West African shipping firm of Elder Dempster & Co., who offered to give £350 a year towards the proposed Liverpool School of Tropical Medicine, as a nucleus. Many sailors and other persons sick with malaria and other tropical diseases were treated at the Liverpool hospitals and especially at the Royal Southern Hospital, near the docks; and such cases would serve for instructing students. Boyce's lecture-rooms and laboratories at the Thompson-Yates Buildings in the College could be used for the teaching, and the Southern Hospital had agreed to join the scheme. Boyce himself was to be Dean of the School (unpaid); Mr. A. H. Milne, of the Liverpool Chamber of Commerce, was to be its secretary; a number of business men in the city, the head of University College, and other influential people, were to form the committee; and Mr. Alfred Jones was to be the chairman. The School was to be affiliated to University College; and Boyce hoped that when sufficient funds and endowments were collected the staff of the School would become permanent professors and teachers of the College. If a lecturer on tropical medicine and a demonstrator of tropical pathology could be appointed at once, the School could open at the beginning of the May term, 1899. One of his own assistants, Dr. H. E. Annett, was well suited for the latter post, and only the lecturer was still to be found. The salary offered him was no less than £250 a year, plus a proportion of students' fees; but the continuance of this salary, much less any final pension, could not of course be guaranteed until it was known what subscriptions were likely to come in. He was to be head of the School staff (one other member), but could not have a seat on the committee. The main object of the School was to teach, but research was to be allowed and encouraged.

On this occasion I travelled to Liverpool with Boyce and we met Mr. Alfred Jones and his partner, Mr. Davy, at Euston Station. I stayed a day or two at Liverpool with Boyce at his charming and beautifully furnished little house, Park Lodge, close to Sefton Park; and we lunched at the University Club, then close to the site of the present cathedral, where I met Dr. C. S. Sherrington, Professor of Physiology at Liverpool (now President of the Royal Society), Dr. Albert Grünbaum, and other future colleagues—a group of keen men of science who took some real interest in my work. I liked them all, and believed that they meant business—though not quite the business that I meant.

My personal position now was as follows. We had been

obliged to spend (including steamship fares) £500, in addition to my salary, during my latter service in India. The pension due to me after eighteen years' service was £292 a year, which, with the Liverpool salary, would be better than regimental pay in India. I might get two or three times as much in a good Indian appointment, and my pension would rise by £200 after seven years' more service; but so might the Liverpool salary. My children were too old to return to India; a good appointment there was very doubtful; and changes of station and fares home would swallow up savings in any case. On the other hand, even if the Liverpool scheme broke down, I was now so well known in the profession that I could take consulting practice in London: in fact, the only reasons why I did not do so at once were (1) That I did not wish to interfere with Dr. Manson's practice there, and (2) that I had still to complete my malaria work by showing exactly how the disease can be prevented in tropical countries—involving more journeys abroad. It was this last consideration which decided me. Liverpool had close connections with West Africa and tropical America; and I thought that the business men there would not be slow to learn the great advantages which my methods of malaria-prevention would confer on those countries and on their plantations, factories, and trade. The teaching-programme of the proposed School was comparatively a small matter; but I could make Liverpool a base for frequent scientific and sanitary missions to the tropics for the purpose of perfecting and applying my ideas. Boyce also had thought of expeditions, but chiefly, I believe, for smaller purposes, such as collecting teaching material.

A few days later, therefore, I applied for the Liverpool post, but at the same time pointed out to Boyce that I was possibly making a serious sacrifice, especially as regards pension.

His reply was:

University College, Liverpool.
School of Pathology, Thompson Yates Laboratories,
9th April, 1899.

Dear Ross,

I will let you know result on Monday (tomorrow) evening as Committee meets a day earlier. I trust that result will be eminently satisfactory to you.

From my own experience I can say that you need not fear as to your future, monetary or otherwise. You will have free control in your own special line.

I will do the Bacteriological part. Room is now being prepared at the Thompson Yates Lab.

It is for you to come here if elected and formulate and arrange matters with us. You will find much ready for you. Unlike London, you will here have the University at your back and you will get from them the *unanimous sympathy of the science men* in authority and that means a good deal. But you will be better able to judge that for youself at the Banquet on the 22nd. With kind regards,

Believe me,

Yours sincerely,

RUBERT BOYCE.

Later on I tried to stipulate that the first charge on surplus funds of the School should be to provide me with some kind of pension on ultimate retirement similar to the Indian pension scheme. Boyce said, reasonably enough, that this could not be done in writing at the time, but that the point would be tacitly remembered. I was appointed to the post by the following letter from the Secretary.

TROPICAL DISEASES COMMITTEE, LIVERPOOL,
B 10, EXCHANGE BUILDING,
11*th April*, 1899.

DEAR SIR,

Referring to your application for the post of Lecture on Tropical Diseases to the Liverpool School of Tropica Diseases, I have to inform you that a Meeting of the Committe of the School was held yesterday when you were appointe to the Lectureship in question at a salary of £250 per annu and a proportion of the Students Fees, such proportion to b determined later.

Your appointment is made subject to the approval c University College, Liverpool, and the Royal Southern Hospita Liverpool, but I am to say that that is merely a formal matte

Yours faithfully,

A. H. MILNE.

MAJOR ROSS, I.M.S., 10, Talbot Road, Bayswater, London.

There was no formal agreement, and it will be observed tha this letter does not even mention length of tenure. I wro again to the Secretary on the latter point, and he finally replied

TROPICAL DISEASES COMMITTEE, LIVERPOOL,
B 10, EXCHANGE BUILDING,
14*th June*, 1899.

DEAR ROSS,

I brought your private letter to me, on the subject of guaranteeing your Salary, unofficially before the Committee of the School.

I am to say officially that the School will definitely guarantee your Salary of £250 per annum for a minimum period of three years, and that at the same time they confidently hope soon to be in a position to guarantee it for a much longer period, (it is hoped as a permanency) and to increase it.

As you are aware, the School is only in its infancy and it is not possible at present to say definitely how much of the financial support accorded this year will be forthcoming annually.

Yours sincerely,

A. H. MILNE.

MAJOR RONALD ROSS, D.P.H., 44 Princes Road, Liverpool.

I quote all these documents in order to show exactly how men of science, especially medical men, are exploited in this country. To no one else in my position would a committee of business men have had the assurance to offer such terms. But my work was to be finished and I accepted the risk. Some months later my pay was increased to £300 a year, but I did not receive the promised professorship (£600 a year) until three years afterwards. Boyce's promise regarding the pension was never fulfilled (page 513).[1] I doubt whether the School would ever have been started, or at least continued, for more than a year or two, if I had refused that risk. The London School was sufficient for mere teaching purposes; but it was chiefly my policy of expeditions and sanitary work which gave to the Liverpool School that fame and prosperity which it enjoyed for ten years or so.

We moved to Liverpool about the middle of April 1899, and everyone was very kind to us. For Boyce (Rubert William), and the memory of him, I shall always cherish the greatest affection. A remarkable man; like a German professor—a Teufelsdröcke; small wizened body; head with long fair hair, grey eyes, aquiline clean-shaved face, resolute lips always

[1] Some years later an Edinburgh veterinary surgeon was persuaded to come to Liverpool on similar promises. He told me that he had been ruined in consequence.

working together in thought; abrupt movements and utterance; one moment seated quietly writing in his office, the next moment gone, disappeared, no one knows where, perhaps for days; like a boy of forty or an old man of fourteen; pulling his forelock while he thinks; not heeding what one says; then a reply like a rapier-thrust! An Irishman, perhaps injured in childhood; choleric, but kindness itself; now trying to arrange his thoughts for a letter or a lecture, now digging feverishly in his garden, or rushing along, coat-tails flying, on a bicycle. He also collected china, furniture, pottery, and tiles; so far as I know had never done important investigations, but knew his work; did not seem to be much interested in science or art, and his faculties were observational and medical rather than calculative. His *métier* really was that of a projector of new schemes, all for the benefit of others, of his laboratory, and of the College. He had innumerable friends among the business men of Liverpool, who knew him (rightly) as *the* professor, and subscribed money for his ventures. I believe it was chiefly he who obtained the funds for the Thompson Yates and other laboratories, for the Liverpool School of Tropical Medicine, and subsequently, for the new Liverpool University—and think that he ought to have been its first Vice-Chancellor. He gave his own money, too, and, I remember, once lost a considerable sum over a students' hostel. He was a bachelor when we came to Liverpool, but in 1901 married a daughter of Mr. William Johnston, a ship-owner. Once a friend called on Mr. Johnston, saying, "I intend to murder that son-in-law of yours." "Why?" asked Mr. Johnston, astonished. "Because he has just got £100 out of me for that —— scheme of his." "Did he ask you for it, then?" said Johnston. "No, indeed," replied the other; "that is just why I am so mad with him. I gave it him without his asking for it at all!" His Hibernian manner was all-persuasive—but he was always sincere. Every scheme of his was the most important one possible.

Similar yet very different was Alfred Lewis Jones, the ship-owner. A Welshman, 54 years of age; rather short, dark, stout, with grey hair, moustache, and tuft below the nether lip, a straight look, ready laugh, and boundless energy. A Lloyd George of business, perhaps a Cleon. Always surrounded by shorthand secretaries, to whom he would fling a letter in dictation while he was talking to you on some other subject and generally followed by reporters and by several people who wanted "half a minute" with him. He gave innumerable

dinners and luncheons, and, before they were quite over, rushed off, secretaries and all, to catch the train for London —which he did just as it was moving away. I saw him dismiss a bore by putting a large banana into each hand of him, spinning him round and pushing him out of the door, both laughing. He found out what you had to say (or thought he did) in half a minute and then instantly proposed a special meeting of the Chamber of Commerce or a deputation to the Colonial Secretary, or some similar move. He gave endless tips and charities, and then complained laughingly that he was being fleeced. Whether he really believed in the mosquito-theory, or even in tropical sanitation, I could never make out ; but I failed in getting him to protect the ports of his own ships by wire gauze against the entry of mosquitoes on the West Coast. He always tried hard to understand me (I think I understood him) ; and no one was more thunderstruck than he when I obtained the Nobel Prize in 1902 (he attempted, though, to get some of it out of me for the School !). He was unmarried, and lived very simply with his kind widowed sister, Mrs. Pinnock, and her children, at Aigburth near Liverpool. His life was written by Alan Milne (H. Young & Sons, Liverpool, 1914).

My friend, Alan Hay Milne, our Secretary, was the son of the Very Reverend A. J. Milne of Fyvie, and was a good-looking young-old man of forty with grey hair, a genial, smiling, diplomatic manner, always confidential but never confiding, a remorseless extractor of subscriptions, a brilliant organiser of meetings and dinners, a wit, and a typical secretary. He was a B.A., Cambridge, unmarried, and a social favourite. Everyone wrote him angry letters or visited him in wrath ; but he met them with smiles and quarrelled with no one.

Such were the men to whom I now entrusted the Great Cause. Above all it required two things—money and advertisement ; and my new friends were likely to give it plenty of both. They were better than the mugwumps of India ; at least they did their best to help.

CHAPTER XX

THE WORK COMPLETED. SIERRA LEONE, 1899–1900

BEFORE going to Liverpool I called at the India Office and told them that I proposed asking for my pension. They advised me not to be in a hurry but to remain on leave for the present; and I did not finally resign until after I heard from Milne on 14 June. My pension began from 31 July, when I was to start for Sierra Leone.

On 7 December 1898 Manson wrote to me to India: "Ray Lankester from the British Museum recognising the importance of the mosquito in human affairs is instituting a systematic investigation of the culicidæ. Through the Colonial Office he has sent out a circular of directions for the collection of mosquitoes with a view of their scientific description by the diptera man at the Museum, and I hope that soon we shall have a better knowledge of these insects." Early in April Dr. Manson and I called on Professor E. Ray Lankester, F.R.S., at the Museum (Cromwell Road). He was out, but I saw him later, and found him interested in my *Proteosoma* work, but sceptical regarding it, and even regarding Manson's *Filaria* work. He said, however, that he was going to Paris at the end of the month, and suggested that I should go with him, so that we might call at the Institut Pasteur together. This was an honour which I gladly accepted. Then he introduced me to Mr. E. E. Austen, in charge of the Diptera at the Museum. I gave him numbers of dead mosquitoes from India, and he explained some points in the classification of the Culicidæ in general. He accompanied me later to Sierra Leone.

The Liverpool School of Tropical Medicine was formally opened on Saturday, 22 April 1899, by no less a person than the great Lord Lister, the inventor of antiseptic and aseptic surgery, and President of the Royal Society (page 315). He was most kind to me, and examined my specimens again with interest. Just as W. E. Henley said of him, "His face at once benign and proud and shy" impressed me greatly—a white-haired, sweet, and patient man, not in the best of health

From that moment a rather long correspondence began between us, in which I explained away his difficulties ; and I have his letters still, with copies of mine to him kindly made for me with Sir Rickman Godlee's permission. The same evening Mr. Jones gave a banquet at the Adelphi Hotel. Many distinguished men were present—Drs. Clifford Allbutt, Church, Manson, Mott, Sir W. Broadbent, Sir R. Thorne, Sir C. Cameron, Michael Foster ; and I saw for the first time Mr. Haffkine and Sir William MacGregor, Governor of Lagos—the latter left before the ceremony. Lord Lister was most kind in his references to my work (see *British Medical Journal*, 29 April 1899, pages 1036, 1046).

A special ward of twelve beds had been set apart for tropical cases at the Royal Southern Hospital, and, of course, I thought that I was to be in medical charge of it. Hence when Lord Lister inspected the ward I asked the House Surgeon to give certain treatment to one of the cases. Next day I received an angry letter from one of the elder physicians of the hospital, Dr. —, for interfering with his treatment of the case. It appeared that I was not to be allowed to treat the cases because I was not an M.D. ! This paralysed the teaching at the School for years, since medical men from the tropics scarcely wished to be instructed in clinical tropical medicine by people who had never been out of Britain and were nowhere known to be authorities on the subject. Verily the modern Briton believes rather in ghosts than in realities.

After the opening I returned to London and stayed with Dr. Manson at 21 Queen Anne Street for a few days. He was very busy with his practice. I dined somewhere, sat next to Professor Ray Lankester, and was toasted ; and on 27 April went with him to Paris, where we stayed at the Hôtel Scribe. Next day we called at the great Institut Pasteur, Rue Dutôt, where I at last saw my master, so long admired from a distance, Alphonse Laveran—grey, thin, alert, with short up-turned beard, glasses, and a manner of absolute decision. We had been corresponding for years with complete comprehension, but now I found it difficult to follow his rapid speech, and when I tried to reply in French found myself always bursting into Hindustani ! There also were Dr. Mesnil, a younger worker at parasitology, and the great Elie Metchnikoff, who had found the rôle of leucocytes as blood-scavengers, and was probably the first to divine the sperm-nature of the " flagella " of *Coccidia* in rabbits (page 251) ; and, later, who constructed a theory of " enteroseptic " disease something like mine regard-

ing malaria (page 92)—a rather big man with a flowing tawny beard. All three accepted my work absolutely, and Lankester was persuaded to do the same. Next day, I think, Metchnikoff gave us a gourmet's lunch somewhere, when he and Ray Lankester talked for two hours on germ-cells. He asked us to a reception at his house ; but I was now obliged to be careful of money matters, and therefore, after seeing something of Paris with so capable a guide as my distinguished companion, returned to London on 30 April. There I saw my mother again, my sister, Mrs. Thomas, my brother Charles, and also Major G. M. Giles—a tall, thin, talkative man (page 330). I was so tired of my wanderings that I was glad to escape even from Paris.

Manson had hinted at all sorts of nice things in store for me on return to England, and Dr. Nuttall wrote me on 19 March from Berlin suggesting " a welcome such as you richly deserve and in which the medical profession should actively participate." I was sceptical, but hoped that at least I should be asked to lecture somewhere on my work, especially at my own old Schools of St. Bartholomew and Netley.[1]

Now, however, I was invited to exhibit my specimens to the Royal Society, and did so at the Soirée held on Wednesday 3 May at Burlington House, but had to arrange for the microscopes myself. By the way, I found that up to date Mr. Baker had sold eighty of my " diagnostic microscopes " (page 131).

The inaugural dinner of the London School of Tropical Medicine was held on 10 May. Chamberlain spoke at it. I was present.

So far as I remember, the summer term at Liverpool commenced early in May, and we had three students. One was Dr. Seved Ribbing, Professor of Medicine at the University of Lund in Sweden—already a senior man, who remained our dear friend until his death there on 14 February 1921 ; another was Dr. van Neck of Belgium, who insisted on joining us later in Sierra Leone. About or before 20 May I gave my first lecture on my work—to the Biological Society of Liverpool, of which Professor Sherrington was President—and was agreeably surprised at my fluency (not always maintained on future occasions). Dr. H. E. Annett assisted me in the teaching as Demonstrator of Tropical Pathology ; and, of course, we were very busy collecting and making specimens, and taking photographs of my *Proteosoma* specimens.

I consulted Professor Ray Lankester on the zoological aspect

[1] Up to the present I doubt whether these institutions are aware of my existence except as one of their " old students."

of the *Plasmodia* in mosquitoes, and on going to Liverpool considered the matter in detail with Dr. W. A. Herdman, F.R.S., Professor of Natural History there. The newly found stages required a new nomenclature, and I suggested one, together with a classification of this group of organisms, in a terse paper in *Nature* [47]. I suggested a much better one in my book on the *Prevention of Malaria* [91] in which all the Greek roots employed (e.g. *-spore*, *-cyte*, *-phore*, *-geny*, *-genesis*) and their various combinations have definite and rigid meanings —surely the rational procedure; but no one took it up, and the confused nomenclature of F. Schaudinn still holds the (long) public ear. We also tried to find infected birds in Liverpool and to ascertain, through Dr. Dawson Williams, the editor of *The British Medical Journal*, whether indigenous malaria was occurring in England—both without success.

With regard to the classification of the organisms, we were bound by the zoological rule that whatever new name be first given to a new species, together with such an accurate description of it as serves to distinguish it from other species, must hold in future, whether the name be good or bad. Now the various species of malaria parasites were often found or distinguished by doctors who did not name them at the time, while other people who gave names often confused several species together. The result has been that names given by subsequent writers, who discovered nothing, had to be finally accepted, with these people's cognomens attached and though the names were badly chosen. The result has been most confusing.

I lost no time in maturing my scheme for expeditions to Africa and elsewhere. Boyce and Milne were opposed at first because they maintained that our duty was to teach, while funds were not plentiful; but I proposed to go during vacation (thus giving up my holidays); and urged that special subscriptions might be asked for. Dr. Annett agreed to go with me, and I made the suggestion officially to the School Committee in writing within a few weeks. I have no copy of my letter, but the reply from Milne is dated 14 June 1899, and gave the Committee's consent.

We were to start at the end of term, and were to receive a small honorarium for additional expenses (I have forgotten how much). The proposal was announced, and aroused great interest. Messrs. Elder Dempster (Mr. Jones's firm) gave us free passages; and Mr. E. E. Austen, Dipterologist at the British Museum, volunteered, and was allowed to come with us. The editors of *The Lancet* and *The British Medical Journal*

asked me to report on our current work, and I agreed to do so, but only anonymously as "A Correspondent," because I thought that my name was being too much advertised at the time. This was a mistake; I sent in four important reports [48], which appeared in September and October and which contained numerous original observations of mine and a detailed account of my new method for banishing malaria by mosquito-reduction; but as my name did not appear as author of the reports much of the matter was subsequently attributed to others. Thus is virtue her own reward—and punishment! Four similar reports appeared in *The Lancet* of the same dates, but, if I remember right, these were written by Annett, though in consultation with me.

On 12 June my inaugural lecture was delivered at University College, Liverpool, in the presence of the Lord Mayor (Mr. Oulton, I think), the Principal (Mr. R. T. Glazebrook), Mr. Jones, Boyce, and many of the staff of the College and the leading citizens. To the disappointment of many I choose for my theme, not my microscope work, but the possible results of it, and the lecture was entitled "The Possibility of Extirpating Malaria from Certain Localities by a New Method." It was published at once [46], at about the same time as my report of 16 February to the Government of India [45]. The lecture now merely amplified the Report; and both enunciated for the first time, in a concise but almost complete manner, the sanitary measure of mosquito-reduction which subsequently cleared Ismailia of malaria and Havana of malaria and yellow fever, and which enabled the Americans to construct the Panama Canal. It is necessary to say this decisively because the fact has been much overlooked.

In both papers I expressed myself as thinking that it would be more difficult to reduce *Culex* and *Stegomyia* (pot-breeding mosquitoes) than *Anopheles* (puddle-breeding). So it often is; but after my visit to Freetown I did not consider it would be so impossible to banish the former as I had thought previously. In both papers I used the words "exterminate" or "extirpate" where, obviously from the context, I meant "reduce." Later I showed mathematically that reduction, in place of complete banishment, might suffice [87 and 91].

We had a very pleasant "send-off" when we left Liverpool on 29 July 1899 in the Elder Dempster s.s. *Fantee*.[1] There

[1] Several kind people gave us nice things, as if we were going into "darkest Africa," and Messrs. Burroughs, Wellcome & Co. presented us with a perfect aluminium medicine chest.

were numbers of men on board who were not happy at going out again to " the white man's grave "—some of them doubtless for the last time. Among them were Dr. Henry W. W. Strachan, Principal Medical Officer of Lagos, who had actually read about malaria and mosquitoes and believed in the theory after I had discussed it fully with him. He was a middle-sized, rather dark man from the West Indies, a good talker and fine performer on the piano, who wrote many letters to me, and who afterwards converted the Governor of Lagos, Sir William MacGregor (he died at Brockley on 13 June 1922).

I kept a daily record of our doings, and find that we reached Grand Canary on 4 August, where we found larvæ of " brindled mosquitoes " (*Stegomyia*) just as in India; and we arrived at Freetown, the capital of the British colony of Sierra Leone, on 10 August. Here I proposed to begin investigations, and we landed and went to stay for a few days with the Acting-Governor, Major Matthew Nathan, R.E., C.M.G. We were armed with credentials from the Colonial Office, and private letters of introduction from Miss Mary Kingsley—that charming West African traveller, whom I had met in London.

Like Port Louis in Mauritius, Freetown is placed on a gently sloping and small plain facing the sea and lying within a horse-shoe-shaped amphitheatre of hills reaching 3,000 feet in height—all covered with brilliant green vegetation. The principal houses are built near the wharves; but from these the suburbs reach backward between the spurs of the hills. Few houses existed on the slopes of the hills themselves, but there were separate villages in the higher valleys above. The soil is derived from a red laterite rock, which lies at little depth and is denuded in many level parts of the town as well as in the water-courses. The rainy season lasts from May till October. The average rainfall is 163 inches, the average humidity 75·0 and the average temperature 80·0° F. The city then contained 30,000 inhabitants, mostly said to be descendants of African slaves liberated from America and placed here by the British afterwards—a large, cheery, negro, Christian race who speak a kind of English as mellifluous as Italian. The houses were mostly poor shanties of wood, often built on raised piles or superstructures of brick and two stories high, with yards or gardens between. The streets were wide (there was almost no wheeled traffic) with badly made gutters, overgrown with vegetation, on both sides. There was a water-supply from the hills with standpipes in the streets; many disused wells;

no sewage drainage; no removal system for night-soil; and three streams from the mountains ran through the town to the sea. Government House itself was only a rambling wooden structure with hot attics for bedrooms. In the middle of the town there is a hill 400 feet high, called Tower Hill, on which there were the barracks and mess of the West African and other native troops, surrounded by a grassy *glacis*. A beautiful place, but famous as the "white man's grave"—though the statistics of the white troops did not show a higher malaria-rate than that of many places in India, such as the Fort at Delhi. The civil sanitation was in the hands of a native municipality—a grave mistake in British administration by which the lives of hundreds of people are sacrificed every year for the sake of political fads.

We were hospitably entertained by the Governor for several days, and were then kindly given quarters with the officers of the 1st West India Regiment on Tower Hill. Fortune now smiled upon me again. I had always said (page 321) that with fairly good luck I should be able to extend all my *Proteosoma* results to the human parasites in a week or two, as the Italians claimed to have done; and that is what happened. We arrived at Freetown on 10 August. Next day Dr. Berkeley, one of the medical officers, sent us dapple-winged mosquitoes of two species, large and small (the medical staff had been previously asked to do this). The small ones came from the Lunatic Asylum at Kissy, where there had been a recent outbreak of fever. I examined five of them on 13 August and found "pigmented cells" in the fifth. It was evident from the characters of the pigment within these cells that they were derived from the tertian human parasite, *Plasmodium vivax*. This small *Anopheles* was a new species very like the few insects in which I had found nothing at Naxalberi a year previously—we named it *Anopheles funestus* (though the suffix *Giles* is now attached to the name). In the meantime we heard that there was bad fever among 400 men of the 3rd West India Regiment serving in a detachment at Wilberforce on the western spur of the hills surrounding Freetown; and as most of these men were Barbadians (where there is no malaria) they were suffering severely. Austen and I went there on 17 August and found the hospital full of cases of all three kinds of malaria, quartan, mild tertian, and malignant tertian, while the walls were dotted with numbers of the large dapple-winged mosquitoes, gorged with blood. These Austen recognised to be *Anopheles costalis*, Loew, which had been described by the

latter in 1866. Next day I began to examine them and continued to examine fresh lots of the same insects brought from the same place, and found malaria parasites in all stages in no less than 27 out of 109 of them, or one quarter! In most of these mosquitoes the species of the parasite was easily distinguishable by the characters of the pigment, which differed in each from that of *Proteosoma*; and all three species of the human parasites were represented. At the same time we found all three species of parasites also in one quarter of the men of the detachment. There were practically no other mosquitoes in the barracks, or birds or animals in their vicinity; so that Fortune had been good enough to provide us with a ready-made experiment at Wilberforce. A list of the positive mosquitoes is given in my final report [52]. Two mosquitoes caught by hand and fed in test-tubes on cases of malaria showed pigmented cells of the proper size; but some insects bred from the larva and similarly fed proved negative for various causes; and the military authorities then stopped further experiments of the kind, though they allowed free biting in the barracks! But ample proof had now been obtained—almost exactly two years after my first success at Secunderabad (page 224); and instead of wasting time over further academical proofs I determined next to study the habits of *Anopheles* in Freetown with a view to the practical measure of their reduction. All this shows how very easy it was to repeat the *Proteosoma* work for the human parasites—as the Italians said they had done at the end of 1898.

On 19 August the first feeding experiment was positive, and I therefore reported our success to the Governor and sent off a telegram to Mr. Alfred Jones, in Liverpool. The telegram copied in my diary was too long, and I actually wired, so far as I can remember, to the following effect: "Malarial mosquito found send help." This was published everywhere at home and, as it was intended to do, tickled the large sense of humour of the great British public, and I believe that even *Punch* mentioned it! We woke up next day and found ourselves famous. Jones wired on the 22nd to my wife, then at Cromer: "Glad say Major Ross cabled yesterday from Leone saying malaria mosquito discovered"; and the School immediately sent out Dr. R. Fielding-Ould to help us, *at its expense.* Milne wrote me on 29 August: "Jones, Boyce, and Co., on receipt of your telegram, went to the Colonial Office and used every effort to make them send out a man at once. We did not succeed as they entrenched themselves behind the fact that they

are sending the Malaria Commission (Drs. Daniels, Stephens, and Christophers) to West Africa." The old story!

When I began to write the promised reports [48] for *The British Medical Journal* I recognised the mistake I had made regarding my self-imposed anonymity. I could not explain the various points without constant reference to my back work and therefore to myself in the third person, so that I was obliged really to puff myself more in this way than if I had written ordinary signed articles. At the same time, myself robbed myself of priority; but it could not be helped now. It was a pity, because the four articles practically summed up, completed, and applied all my previous work, and did so in a much more lucid and interesting manner than did my final and formal report published in February 1900 [52] (page 394). The diagram of mosquitoes seated on a wall in the third of these papers was done by me, but has been copied, with additions, by many original workers.

On 22 August, Dr. van Neck, a Belgian student of the School, arrived on the scene and wished to attach himself unofficially to us; but he lived in the town and was soon taken ill. We had originally hoped to proceed farther down the coast, but our success at Freetown kept us there studying the bionomics of *Anopheles* and making a map of their breeding pools for the future use of the Colony. At the request of the Governor, I presented a report to him on our work, advising what should be done to prevent malaria, and we left Freetown for home by the s.s. *Fantee*, on 27 September and reached Liverpool on about 3 October.

The people of Freetown have their points—somewhat like black Irish; merry, full of life, and extraordinarily musical. The singing in a church on Sunday, performed by numerous large perspiring people dressed in black coats and top hats or in copious flowered gowns, can be heard in the hot mornings a mile away. When we searched roadside puddles the big lady of the neighbouring house would come to her garden gate and ask: "Wha' fo' you look in da watah dah, mastah?" To this we tried to reply in the vernacular: "We look fo' da mahskeetah in da watah, sistah." After a moment's pause of astonishment at anyone being such a fool (as a white man) to look for mosquitoes in water, she would open her copious mouth and shout out for all in the street to hear: "Ha, ha, mastah he look fo' da mahskeetah in da watah, ha, ha, ha!"; and all the neighbouring big ladies, who had come by this time to stand also at their garden gates, would join in the chorus.

I now conclude this chapter with a letter which I wrote on our way home to Lord Lister, because it summarised our results while they were fresh in my mind.

S.S. "FANTEE,"
2nd October, 1899.

MY DEAR LORD LISTER,

Very many thanks for your letter of the 5th September which we received before our leaving Sierra Leone. I had intended to give you our further results regarding malaria before but finally determined not to trouble you too much with letters until our work was complete. We are now on our way home again, and I sit down to tell you our final conclusions.

I said we had found human zygotes in two species of *Anopheles*. The fever among the troops at Wilberforce continued to yield naturally the most perfect evidence regarding the diffusion of malaria. We found that about 25% of the men had Hæmamœbidæ, mostly in small numbers, in their blood, though very many of them frequently took quinine. The only species of mosquito present was the large variety of *Anopheles*, and out of 108 of these which were carefully examined we found the same parasites in their various stages in 26. Unfortunately when we first went to Wilberforce we caught many more of the insects than we could examine, that is, we killed nearly all the *old* ones. Later we found that fresh insects arrived only in small numbers, and of these fresh insects a smaller percentage, as was to be expected, were found infected after feeding. The total gives about 25% of infected insects—quite sufficient to account for all the fever in the Wilberforce barracks. We were very fortunate in obtaining such a remarkable object lesson, which seemed to have been arranged by Nature herself for our instruction.

As soon as we were satisfied that the local *Anopheles* carry malaria, we threw all our efforts into studying the habits and distribution of the larvæ. Things turned out just as I said they would in my inaugural lecture. We found that *Culex* breeds in pots and tubs of water, *Anopheles* in pools on the ground. Any old vessel, broken bottle, empty gourd or biscuit-tin suffices, when full of rain-water, for *Culex*; but only in a single instance did we find *Anopheles* in such and that was in an old tub full of green water-weed. At first we had con-

siderable difficulty in finding *Anopheles* larvæ at all, but we shortly learned how to detect their pools at sight. The results obtained are, I think, most interesting, both theoretically and practically. Only certain kinds of puddles suit the larvæ. Puddles which dry up too quickly, which are apt to be scoured out by heavy rain, and which contain small fish, do not suit them. We kept a number of puddles under constant observation in order to study the habits of the insects. It was most interesting to observe that while one puddle would always contain larvæ, another one close to the first would never contain them. The reason was that the first would fulfil these conditions and the second would not do so. A large number of pools in Sierra Leone are apt to be scoured out by the torrents of rain which fall there; another larger number dry up within a few hours after rainfall, while a few of the larger pools contain little fish. The upshot is that comparatively few puddles are suitable for the larvæ, and these are situated either in the ditches by the side of *flat sections of road*, or in two localities where small runnels of water ooze from the ground and run over flat areas of rock or soil containing numerous small hollows. In the latter localities the puddles, being fed by the runnels of water, are almost permanent during the rains, and contain masses of algæ. Here the larvæ exist in great numbers. In fact they eat the algæ, as was to be seen at once on a microscopic examination of the contents of their stomachs, as well as by watching the little creatures feeding. We ascertained, both by experiment and by observation, that complete dessication of a puddle kills the larvæ, though if sufficient moisture remains they are uninjured. As rain falls almost every six hours at Freetown during the wet season, it follows that very small pools can harbour the larvæ during this season. On one occasion, however, we had four fine days. Nearly all the puddles dried up. Then came heavy and continuous rain and the puddles filled again; but at first the freshly filled pools contained no larvæ, and it was not until 48 hours that we found larvæ again—all being extremely small ones, evidently just hatched from eggs laid by the adult *Anopheles* living in the neighbouring houses.

It appears to us that these laws give an almost complete explanation of many familiar theories regarding malaria—

such that it springs from stagnant puddles, that it is connected with rainfall (in Freetown the height of the rains is the most malarious season), and that it disappears in consequence of drainage of the soil. You will remember the theory that malaria can rise from decaying rock. It was most remarkable to see the larvæ in small pools in the hollows of certain flat, worn old rocks. Again, you will remember the theory that digging the earth causes malaria. This had actually been noticed at Freetown during the construction of the railway. On examination we found that the railway embankment had produced *Anopheles* pools here and there along its course! It is easy to see that digging the earth may often produce holes which when filled with rain-water may harbour *Anopheles*. The old theories were then fairly right after all; but it is not the malaria germ itself which rises from stagnant water, but the *carrier* of the germ. Had *Anopheles* bred in pots of water like *Culex*, the old theories would have remained unsatisfied. On the other hand, the fact that *Anopheles* breeds in pools seems to my mind to reconcile the mosquito theory almost completely with known facts regarding malaria—and does so in a particularly beautiful manner.

Of course these observations almost cut the ground from under the feet of the argument that malarial fever may be produced in other ways than by the bite of the mosquito—they seem to me to give a sufficient explanation of the facts upon which that idea was founded. It is scarcely necessary any longer to imagine that the germ rises from the soil and is inhaled and so on; while on the other hand such a theory would imply another life-cycle in the Hæmamœbidæ—whose history is already complicated enough. One might as well argue that tape-worms are acquired in other ways than by eating flesh containing cysticerci.

But two important questions remain. First, can any *Culex* carry human malaria? This I fear we have made no attempt, on account of limits of time, to answer. The R.A.M.C. threw difficulties in the way of our making mosquitoes bite their patients; so that we were obliged to abandon experiments in this line. But a systematic study of the question is certainly demanded. We found several species of *Culex* in puddles together with *Anopheles*, though the larvæ were not nearly

so numerous. At any rate I am far from being convinced yet that *Culex* is quite innocent.[1]

Secondly, can malarial fever be acquired in uninhabited places? You discussed this in your letter of the 31st July and suggested that animals may be intermediary hosts. After all, there is so little evidence to show that malaria can be acquired in uninhabited places. Travellers, for instance, must pass through inhabited villages before reaching deserts and can easily be bitten by infected mosquitoes *en route*. But I quite agree that it may be possible, and also that animals *may* be vicarious intermediary hosts. At Wilberforce there are no animals at all. Here simple communication by the *Anopheles* from man to man suffices to explain all the fever, we think; and if it suffices there why not elsewhere? By the by, we found no *black spores* except one doubtful instance, in all the Wilberforce *Anopheles*. My doubts regarding these bodies have grown graver than before.

Now for the practical results to be expected as regards the possibility of extirpating *Anopheles*. We all agree that if the local authorities will set themselves heartily to the task, Freetown can be freed of the insects at a trifling cost. We carefully made a map of the whole town, giving all the *Anopheles* puddles. There are only about 100 of these all together, lying mostly in clusters. All could be drained at little cost and most could be swept out with a broom. We tried kerosine oil; it was quite successful. Dr. Ould is now trying tar. In fact we can see no difficulty in destroying the insects wholesale, and if such destruction were kept up for a few months it seems probable to us that the prevalence of the insects in Freetown would be dealt such a severe blow that it would take them a long time to recover their ascendancy. The Governor, Major Nathan, has been so good as to adopt our views and has organised measures against the *Anopheles*—but of course we cannot answer for the carrying out of these measures.

Dr. Strachan, Chief Medical Officer of Lagos, who travelled out with us, has found hosts of *Anopheles* there. He has also discovered the larvæ in roadside puddles and is destroying them with oil. From specimens which he has sent us, they

[1] I am still not quite convinced. Very little real work on the subject has been done since then. Negative propositions are hard to prove.

are the same as the Sierra Leone *Anopheles*. We have also obtained a similar insect from Opobo. Hence it appears probable that this insect is the principal cause of the West African fever. Dr. Ould, who arrived at Sierra Leone a fortnight ago, is now going down the coast to *raise the alarm* against *Anopheles* and will then work at Lagos with Dr. Strachan. I firmly believe that a general massacre of *Anopheles* in the principal coast towns would be quite practicable and would result in at least a considerable reduction of the fever there.

Austen was attacked with the æstivo-autumnal parasite, but has recovered speedily. He slept without mosquito-nets about a fortnight previously (for one night) but does not remember having been bitten. Of course one may be often bitten without knowing it.

In your letter of 31 July (which I have not yet fully answered) you refer to the gametocytes of *H. Danilewski* (Halteridium) of pigeons, jays, etc., being crescentic, while I place this parasite in the genus *Hæmamœba*. I characterise this germ by the gametocytes being of the same shape as the sporocytes (whatever that shape may be); while in *Hæmomenas* the gametocytes have *a special* (and, as it happens, crescentic) shape. In *Hæmamœba danilewski* of jays, etc., both sporocytes and gametocytes are crescentic certainly; but they are both alike, and their shape is evidently due to exigencies of space. In *Hæmomenus præcox*, the sporocytes are spherical, while the gametocytes have quite a different shape—truly crescentic. It was difficult to retain the word Laverania.

I am sorry for the slip about the date of MacCallum's discovery. The hint about cultivating the black spores is very good. I will write to Daniels about it.

You will understand that there will be some difficulty in moving the *inertia* of the medical profession in these parts in the direction of destroying *Anopheles* larvæ. We have thought it best to advertise the discovery of the "malarial mosquito" somewhat freely on this account. I hold that the subject is one of those with regard to which the public should be taken into confidence. Unless this is done progress, I am convinced, will be very slow. For example, the microscope is not even yet habitually employed in the diagnosis of malaria

fever in our tropical colonies, although microscopic diagnosis is easy and in many cases indispensable, and Laveran's discovery has been made for twenty years.

I hope Dr. Strachan at Lagos will take energetic measures against *Anopheles* there. He seems interested in the subject and will have Dr. Ould to help him.

I fear I have written you an inordinately long letter, but think that you may like to have pretty full details. Our report will not be ready for some time.

Want of time has prevented us from doing any good cytological work. The three human parasites are distinguishable by their pigment in the zygote stage—that of tertian being light brown, of quartan, dark brown, of æstivo-autumnal, black. The capsules of all seem to be a little thinner than the capsule of the zygote of *H. relicta* (Proteosoma). Otherwise there seems to be no difference at all. The blasts [germinal threads] seem shorter and thicker and can often be seen within the lumen of the duct of the salivary gland.

I have reported our results also to Professor Ray Lankester, Dr. Manson and Dr. Laveran.

We are trying to bring some *Anopheles* home with us alive, but I fear they will all die before our arrival.

Believe me,

Yours sincerely,

RONALD ROSS.

I was much disappointed that even now, owing to lack of time, I was not able to apply the Romanowsky method of staining to my cycle of the malaria parasites. This required *fresh* specimens and could not be done at Liverpool. It would have been quite easy if we had been able to spare a few weeks more at Sierra Leone; but we were obliged to hurry back for the autumn term at Liverpool. My work was therefore left imperfect as regards cytological details; but, really, we could not be blamed for this, and the Italians had already done the cytology.

In October Dr. Fielding-Ould proceeded to the Gold Coast and Lagos, and his report was printed together with mine (page 394). He was able to find the "pigmented cells" in some more *Anopheles*.

Shortly after our return to Liverpool Mr. Alfred Jones gave us another banquet; and on 27 November 1899 I delivered

a lecture to the Liverpool Chamber of Commerce on the subject of our expedition, in which I detailed the political and economical bearing of our work. There was a rather fiery denunciation of public indifference to the investigation and prevention of disease (but a perfectly justifiable one); and after quoting Tupper's line about Columbus, that he "gave to man the godlike gift of half a world," I added, "Similarly it may even happen that such a wild idea as killing mosquito-grubs to prevent malaria may assist in giving to civilisation the gift of another half a world—the tropics. We never know, when we plant one of the seeds of science, into how great a tree it may grow some day." All this matter has been much repeated by others since then, but, I fear, has been little acted upon.

Nuttall showed later [51] that many of our observations regarding mosquitoes had already been recorded, at least partially, in previous entomological papers which were forgotten or unknown to us. This is what often happens, and is of little consequence. We were the first to bring all the facts together into a single body of sanitary doctrine for the general prevention of mosquito-borne disease. Mere observational science frequently proves useless—the facts have to be rediscovered when some subsequent generalisation requires them.

The arguments already cited in favour of active Imperial action with regard to malaria were now redoubled in strength. Britain was given an opportunity such as few nations had ever enjoyed—an opportunity of showing her capacity to rule by reducing on a large scale the disease which was such a pest to millions of her subjects and to thousands of her officials. Not only had the general laws been discovered, but two species of the death-bearing insects had now been detected in West Africa, their habits ascertained, and the methods of dealing with them accurately defined. What more was required?—simply to do the work indicated. On the voyage home, I hoped for, and expected, an immediate conference of the great Government Departments in London which were concerned with the subject, and believed that within a few months we should have not one but fifty men working in West Africa alone the lines which I had suggested. In the Colonial Office, with Manson as its Medical Adviser and Joseph Chamberlain as its head, we possessed an immense force for progress, and the former had warmly commended the Sierra Leone work and had written to me (12 October): "You have held up the torch once more and I heartily congratulate you." Behind us there

was Liverpool, with its numbers of rich merchants connected with West Africa, to see that no time should be wasted; and, weary though I was of the tropics, I expected to be called to the front again at any moment—this time in charge of a worthy army.

I do not remember the date, but I think [1] it was shortly after our return from Sierra Leone that my former chief, Surgeon-General Harvey, came to see the School at Liverpool. He was very kind about my work, but seemed hurt at a paragraph which had appeared in some paper blaming the Indian Government for not helping me more, and he assured me that "an honour" would be in store for me if I would withdraw this statement! I told him that the paragraph had not been even "inspired" by me and that, on the contrary, I was thankful to him and to Surgeon-General Cleghorn for having put me on special duty (page 255) for the work at all; and this *was* certainly something surprising—for India! He then proceeded to suggest that I should return to India and that I should be given a good and permanent salary and post—everything, in fact, that I had wished for a year previously. Alas—as I pointed out to him—it was too late now, because it would not be fair to leave Liverpool in the lurch after a few months in this way, although they were giving me such poor pay still. He left the matter open, and went on to express his regrets about the kala-azar interruption (page 356); and, in fact, behaved so handsomely that, *as an individual*, I forgave, and forgive, it all—but there is also our duty to *science*. I have no letters from him at the time, but possess a long one from Aberdeenshire, dated 28 August 1901, in which he states that he was still trying to get me the Indian honour! [2] The matter of my returning to India was opened again very soon, and on 14 February 1900 Manson wrote to me: "I had a long interview with Leslie (Harvey's secretary) the other day. . . . I reproached him with allowing you to leave India without due reward and without an effort being made to retain your services. I think they are conscious of their shortcomings in these respects, and I think they are penitent. In fact Leslie said they would have you back again tomorrow and give you anything

[1] I see by a letter from Dr. F. Maynard, I.M.S., dated 13 June 1899, that Harvey was then in England. He was also certainly in Liverpool again in June 1901, when I was leaving for my second expedition to West Africa (page 440). He died about the end of 1901, I believe.

[2] When I was at the War Office in 1918 I heard a curious story about myself from India. It was said that I had been offered a Kaiser-i-Hind Medal (a government honour) but that I had replied refusing it but enclosing two rupees for a C.S.I.! The discourtesy—and I fear the wit—were beyond me!

you liked to ask for in the way of work and a good salary." Of course, I could only refuse again. Nevertheless, when I applied in 1912 to Lord Crewe for an ordinary "good-service pension" he rejected the application (page 505).

We had few students in the autumn of 1899, but I was busily engaged in writing my report on Sierra Leone, besides several other papers. My first care was to bring out a pamphlet [49] called *Instructions for the Prevention of Malarial Fever, for the Use of Residents in Malarious Places*. It was published as Memoir I of the Liverpool School of Tropical Medicine in 1899, and contained ten sections and fourteen pages. Again, for fear of too much *réclame*, I made it anonymous—from which it appeared that the matter of it was all old familiar stuff, whereas, really, it consisted almost entirely of my own recent observations. It sold rapidly and reached a sixth edition in 1901. I sent Manson one of the first copies. He approved of it warmly, but suggested (14 February 1900) some verbal amendments which certainly bettered my hasty style, and on 16 March added that if the amendments were made the Colonial Office would be likely to "take it up"; that is, I suppose, to supply copies to their employees going to Africa. Boyce and Milne, however, thought that this could not be done owing to the expense, so that, I believe, the Colonial Office did not adopt the work as suggested. I retained the copyright and received some small royalty; but the School got the credit, of course. On 2 December 1901, not content with this, Jones actually tried to force me to promise all my future works to the School and to advertise that institution in them! At the same time I had rewritten the *Instructions*; and the ninth edition of the book was published, still as Memoir I, and by the University Press, in February 1902, under my name and with the new title, *Malarial Fever, its Cause, Prevention, and Treatment* [66]. This was really another book, which contained thirty-two sections, two plates, and sixty-eight octavo pages. It is still selling, and has been translated into German (Wilhelm Süsserott, Berlin, 1904) and into modern Greek (Ethnikoy Typographeioy, Athens, 1906).

Before the end of 1899 I contributed an article [55] of fifteen pages on Malarial Fever to the well-known *Medical Annual*, published by Messrs. John Wright & Co., Bristol, for 1900 (some misprints and other slips). On 16 March Manson wrote to me:

"Many thanks for your reprint from *The Medical Annual*. It is a first class piece of work and I am very grateful to you for

the full justice and more than justice you have done me as regards my very small share in the mosquito-malaria theory."

On Thursday, 4 January 1900, I lectured in connection with the Livingstone Exhibition at St. Martin's Town Hall, Charing Cross.

In December 1899 Dr. Manson told me that he was about to visit Italy and hoped to go to Rome to find out what they were doing there. Accounts appeared in the medical press and in *The Times* of 30 January 1900, page 12, from which we heard that he was one of a party of physicians, including Dr. Launder Brunton, Sir Clifford Allbutt, Dr. Cantlie (editor of *The Journal of Tropical Medicine*), Dr. Malcolm Morris, and others, who had been invited there to see a hygienic institute in Sicily (I think), and that the party had remained some days in Rome in conference with Professors Grassi, Bignami, Celli, and Bastianelli on the malaria question. After mentioning Grassi's great discoveries in Catania regarding eels (page 399), the (anonymous) article added that, " Professor Grassi, since his removal to Rome, has performed work perhaps no less remarkable in demonstrating the propagation of malaria by mosquitoes." My own humble labours consisted only in some trifling studies on the malaria of birds : " Himself an ardent engineer (*sic*) and now chief of the Liverpool Tropical School, Major Ross set to work with some success, but unfortunately his efforts were impeded by the active discouragement of his official superiors in India." The writer generously gave me credit, however, for having been able to " verify " the great Italian discoveries during my visit to West Africa !

Of course I do not know who wrote this effusion—probably one of the doctors of the party ; but it angered us in Liverpool. We expected that Manson would have instantly corrected it —he knew all the facts and I had repeatedly given him the whole credit of my work in India. But I suppose he thought otherwise and remained silent ; so that after a few days I was obliged to send the following letter, which appeared in *The Times* of 7 February, page 12 :

MALARIA AND TUBERCULOSIS

To the Editor of " The Times "

SIR,

After a man has spent years over the solution of a difficult scientific problem, it is always gratifying to him to find the credit given, not to him, but to those who have performed the easy task of repeating and confirming his work.

An Englishman must especially guard against this kind of thing, because, as we all know, our countrymen always like to give the credit to a foreigner if they can. For example, Dr. Manson and myself succeeded (after years of labour on my part) in following out the life history of the Hæmamœbidæ (the group of parasites of which the human varieties cause malarial fever) in mosquitoes. On perusing the English papers, however, I now find that this was done, not by us, but by some Italian gentlemen who, as a matter of fact, did not commence their labours until months after ours were finished, and whose work is simply an imitation and continuation of ours. For instance, the writer of the article with the above heading in your issue of January 30 is kind enough to emphasize such erroneous views. This he does by making out that I performed only the humble feat of cultivating the parasites of birds in mosquitoes; that I failed in cultivating the human parasites; and that the Italians did all the important parts of the work: while he actually goes on to attribute to them numerous observations made by me long before they even began any sound investigations on the subject, and then talks about my work confirming theirs!

As a matter of fact, I cultivated the human parasites nearly a year and a half before the Italians, while the cultivation of the parasites of birds, which your Correspondent makes so light of, completed the general solution of the problem. All that the Italians did was to apply this general solution to particular cases when I myself was prevented by interruptions from doing so—a work of no difficulty whatsoever. Even in this they must share the credit with Professor Koch, who did the same work almost simultaneously.

Sir H. W. Bliss has already corrected your Correspondent's statement that my labours were impeded by the "active discouragement" of my official superiors in India. On the contrary, the Government of India, partly at the instance of Sir H. W. Bliss, gave me great assistance. I suffered from frequent interruptions, it is true, but these were not due to any active discouragement.[1]

Yours faithfully,

RONALD ROSS.

February 5.

[1] I *hope* that this is true.

On 9 February *The Times* (which has always helped me warmly) made amends in a handsome leading article in which it also mentioned my *Instructions for the Prevention of Malarial Fever*; and the author of the twaddle gave a pusillanimous reply on 13 February.

About the middle of 1899 Professor Angelo Celli of Rome had written a book on *Malaria According to the New Researches.* An English translation of the second edition of it appeared in 1900 (Longmans, Green & Co.). The author was gracious enough to describe my work on avian malaria and even to call the cycle which I had discovered "Ross's cycle." But, as usual with these Roman writers, there is no accurate bibliography, there are many prevarications, and the history is so confused that an ignorant reader is impressed only with the magnificent Italian discoveries. In his preface he actually hints (as the writer in *The Times* had done) that my work in Sierra Leone was plagiarised from his countrymen—the thieves accuse the owner of theft! The book contained nothing original except perhaps some facts about Roman malaria, but much was taken from my inaugural lecture and my Sierra Leone articles in *The British Medical Journal.* Somewhat to my surprise Manson gave an Introduction to the book. It was well written, but if I had been he I would have declined the honour.

Meantime I had completed the *Report of the Malaria Expedition of the Liverpool School of Tropical Medicine and Medical Parasitology* [52]. It was written entirely by myself, and Boyce wished that my name only should appear on it, but I insisted that those of Annett and Austen should be added. It was dedicated to Mr. Joseph Chamberlain (I wonder whether he ever read a line of it). At that time the South African War had commenced, and Austen, who was in the Artists' Rifles, had gone there, leaving the descriptions of *Anopheles costalis* and our new *Anopheles funestus* to Giles. The plates included eighteen microphotographs of the mosquito cycle of some of the human and avian *Plasmodia*, made by myself, and excellent drawings of the *Anopheles* made at the British Museum. There were also scenes in Freetown, together with my drawing of the respective attitudes of *Culex* and *Anopheles*, and a plate (Plate IV) by Fielding-Ould.[1]

[1] Dr. Fielding-Ould's plate was not unlike one by B. Grassi, which, in its turn, was far from unlike my *Proteosoma* drawings—with artistic embellishments. Because the first was published by the Liverpool School, together with my Sierra Leone Report, Grassi now accused me of plagiarising his drawings!

Dr. Laveran reviewed both this Report and my "Instructions for the Prevention of Malaria" (Memoirs II. and I.) in the *Bulletin de l'Académie de Médecine*, Séance du 3 avril 1900, and gave an account of the founding of the Liverpool School.

Now a very pleasant and honourable duty fell upon me. As long previously as 13 October 1899, that veteran and orator of science, Sir James Crichton Browne, M.D., F.R.S., invited me to give a Friday-evening discourse before the Royal Institution in London on 2 March 1900. The famous lecture-room in Albemarle Street was packed, and the Duke of Northumberland, who presided, escorted my mother and wife to the seats of honour before which so many a man of science and literature had spoken in the past. I had prepared carefully a written lecture covering nineteen close octavo pages—which summarised the whole history, Manson's induction, my work, the Italian work, and the Sierra Leone work; but it was too long to read and I therefore said what an unpractised speaker could say in the time. Afterwards a number of my specimens were shown; and the lecture was printed by the Institution [53], by *Nature* (29 March), by the Liverpool School as a pamphlet, and was, lastly, translated into French and published in the *Revue Scientifique* (23 June). On Monday 28 May 1900 I gave a similar lecture at Cambridge, ably organised by Dr. G. H. F. Nuttall (page 334), who was now working there. I dined with Dr. Shipley, F.R.S., at Christ's, and with Professor Sims Woodhead next day after the lecture; and found much wisdom and some good living at Cambridge!

On 8 November 1899 Professor Ray Lankester suggested that I should give him a series of diagrams of the whole life-cycle of the *Plasmodia* for his *Quarterly Journal of Microscopical Science*. The diagrams were done by me, then fair-copied by Fielding-Ould, and finally appeared on three full-page plates [56] in July 1900, with a concise description of the cycle, written by me, but under our joint names, and a Note by Ray Lankester. Some of the drawings are given in Plate II, page 115.

All this was very pleasant—famous men, famous places; but, alas! what were the mosquitoes doing in India and Africa? What had happened to my great projects? While I was talking, arguing, and explaining, the millions were still suffering there just as before. Was I not betraying the duty that had been laid upon me?

CHAPTER XXI

ROMAN BRIGANDAGE. 1900

BUT I must postpone the history of this drama till my next chapter on account of a humorous interlude which will now be presented by a professor of zoology at Rome in his last heroic effort to discover the "*Anopheles malariferi indipendentemente da Ross*"—and of his own colleagues! The pantomime is not agreeable to me; but I have set out to give an exact account of a medical investigation, and a part of the duty lies in showing what may happen to the investigator, and also how easily spurious science may be mistaken for genuine science. The story has never been fully told in English,[1] but I will now reconstruct it in detail from unpublished papers of my own and from some published records (the reader may omit it!).

At that time a group of Roman writers on malaria and similar subjects had long been notorious among other workers on these themes for some of their qualities. They had never had the good fortune to make any of the leading modern discoveries on malaria, which, as usual, were obtained by isolated individuals. Meckel discovered the "malarial pigment," Laveran the parasites, Danilewsky the first parasites in birds, Golgi of Pavia the different species and life-cycles of the *Plasmodia* in men, and Romanowsky the correct method of staining them. But as the Romans possessed immense opportunities for the study (page 341), they were always able to fill in the details of the more fundamental discoveries made elsewhere. This they did in numerous monographs and articles written in the most beautiful and easy of languages, admirably printed, and illustrated by exquisite coloured plates which appear to be beyond the means of science in less civilised countries. But their writings generally showed (in my opinion) two of the greatest defects that scientific writings can show: the history of the investigation was sure to be inaccurate or even falsified,

[1] I gave the broad facts in [78] and [91], but numerous writers, especially English ones, continue to repeat the old falsifications just as before—which compels me to write this chapter.

and the recorded work gave evidence of hasty efforts to snatch at priority and frequently proved ultimately unsound.

Amusing instances of all this are described in the historical chapter of the successive editions of Laveran's great book, the *Traité du Paludisme*. About the middle of last century many enthusiasts thought that they had discovered various marsh-living vegetable organisms which were the cause of malaria; and, accordingly, in 1876 the Roman writers Lanzi and Terrigi discovered the *Bacteridium brunneum* which they said produced the malarial pigment in the spleen; while in 1879 Tommasi Crudeli and Klebs found the still more remarkable *Bacillus malariæ*. They isolated the latter from the blood; they found it in the water and mud of marshes as well as in marsh air, especially the lower strata—just where they expected it to be; and, on injecting it into the blood of rabbits, produced the fever, the enlargement of the spleen, and even the pigment, all so characteristic of malaria! This discovery was fully "confirmed" by several distinguished Italian writers after numerous valuable researches, and was then soon accepted in Italy, Germany, and England, where translations of Crudeli's work appeared—see especially vol. cxxi. 1888 of the publications of the New Sydenham Society. When in 1880 Laveran discovered the true cause of malaria these distinguished Italian writers would not believe him at all; but when, about 1887 and subsequently, Golgi finally clinched the proof of Laveran's discovery, they were forced to abandon Crudeli; and then some of the Roman writers immediately pretended that Laveran had merely seen the bodies which he had described, but that it was they themselves who had really proved these bodies to be parasites and to be the cause of malaria.[1] Similarly they tried to absorb the credit of Golgi and of several others of their countrymen; and they continue these pretences to the present day. Their method is as follows. When some patient investigator has announced some new fact they repeat a few of his investigations—which with the abundance of material they possess is generally an easy matter—and then, without waiting to corroborate details, begin to pour forth a flood of literature, generally written in collaboration in order to render it more credible. No consecutive history or exhaustive bibliography is given, and there are many old hypotheses which appear to be new and brilliant to the majority of readers—

[1] They said and still say that they did this by showing that the malarial pigment is produced within Laveran's bodies. But anyone can see this for himself.

who cannot be experts. Their own few observations are seldom given in detail but are merely hinted at in general and grandiloquent language—see page 351 for an excellent example. Next they proceed to misrepresent the work of the original investigator, to find it to be full of errors, and then to correct him, and so, quite reasonably and justly, to claim his credit. These works are then translated into German, French (seldom !), and English, and are sown broadcast in the form of offprints through every scientific laboratory in Europe and America. The falsifications are next embedded in the learned textbooks of the day—mostly written by people who find it easier to copy from previous textbooks or recent *brochures* than to consult old original papers ; and, finally, the meed of discovery in the form of prizes, fellowships, and honorary degrees falls deservedly on the great Signor So-and-So. Meanwhile—as there are probably not six persons in the world who really know the facts—the true discoverer languishes, for a time at least, in the shades of oblivion.[1]

How deserved this criticism is can be known only to those who, like myself, have studied the literature ; but the reader will find clear verification in Chapter XVIII. An ingenious example of *suppressio veri* by Bignami is exposed on page 340, and a clumsy example of *suggestio* (or rather *affirmatio*) *falsi* by Bignami, Bastianelli, and Grassi is accurately named on page 351. I have grounds for believing, however, that it was the last-named gentleman who was really responsible for the last-named delinquency.

In a famous passage in his essay on Warren Hastings, Macaulay said : " What the Italian is to an Englishman, what the Hindoo is to the Italian, what the Bengalee is to other Hindoos, that was Nuncomar to other Bengalees." And that, I may add, was G. B. Grassi to his colleagues, his colleagues to other Italians, and Italians to other scientists. He has been engaged in many disputes on matters of priority. I possess several remonstrances by Dr. Ernesto Parona of Milan regarding Grassi's treatment of him in connection with researches on *Bothriocephalus latus*. In 1900 the late Salvatore Calandruccio of the University of Catania published a paper entitled *Unicuique suum, prof. J. B. Grassi*, in French (C. Mariani, Roma), in which he made many similar complaints.[2] For

[1] Dr. Calandruccio, himself a victim, remarked that though his countrymen had succeeded in putting down brigandage under arms they had introduced a new kind of brigandage in its place which consists of robbery in science.

[2] English translation in *Journal of Tropical Medicine*, London, 1 July 1901.

example, he avers that he found *Filaria recondita* of the dog in ticks; but that Grassi published a *résumé* in a German journal with his own name inserted as a collaborator with Calandruccio. Grassi used the same device, he said, in connection with researches by Calandruccio on *Ascaris lumbricoides* and *Trichocephalus dispar*. Calandruccio discovered an Arachnid; he and Grassi studied it together; Grassi, however, published the investigation without mentioning Calandruccio's name. Calandruccio says that he did most of the work (lasting seven years) in connection with a memoir on Termites; Grassi in publishing the memoir wrote: "I thank Dr. Calandruccio for the aid which he has given me in this work." Calandruccio says that he found the larvæ of *Filaria immitis* in mosquitoes, and then ascertained that dogs are the definitive hosts; but that Grassi then attributed this discovery to himself. Lastly, Calandruccio says that it was he who laboriously discovered the transformation of eels (a very important matter) at a personal cost of 6,000 francs; but it was Grassi who received the award of a medal and prize for it and the work on Termites from "l'Académie de Londres." Yet, as I know, the work was done and published in collaboration—I have papers referring to most of these points. It would appear that during this period Calandruccio was in such a relation to Grassi that he could not defend himself with effect. Since then I have received several similar criticisms of the same philosopher, notably by Prof. Guido Cremonese of Rome and by Prof. Claudio Fermi of Sassavi—altogether a very unedifying spectacle, not conducive to the honour of science.[1]

Regarding the mosquito-theory of malaria, Grassi, like most of the Romans, remained a complete disbeliever until I had proved the theory and Manson had published the proof in *The British Medical Journal* of 18 June 1898. Of course, when the parasite of malaria was discovered in 1880, everyone surmised that the extra-corporeal form of it must inhabit marshes. It was therefore necessary that the distinguished Roman students of malaria should find it in marshes. This was easy to Grassi, who quickly discovered it in the form of a fresh-water amœba—*Amœba guttula*. At the same time Grassi and Feletti invented the hypothesis that the so-called

[1] When Low and James found the embryos of *Filaria bancrofti* in the proboscis of mosquitoes in 1900, Grassi at once tried to pick holes in their work and then to absorb the credit for himself. See my letter in *The Lancet*, 13 September 1902, page 775.

flagellate bodies are " agony-forms "—that is, bodies dying in the exposed blood (*Sui parassite della malaria ; Riforma Medica*, 1890, page 62)—and their opinions were accepted by Marchiafava, Celli, Bignami, and others. They also thought that the flagella contain no chromatin, and Bignami ridiculed Sakharoff for saying the opposite [21]. Not only were all these views wrong, but they must have been founded on the most worthless evidence. More probably they were all mere plausible conjectures such as one often finds in the writings of these gentlemen—conjectures emitted without any experimental evidence, simply on the strength of what appeared to be likely at the time—to be subsequently abandoned if proved wrong, but to form the basis of claims for priority if found right. Grassi also decided that mosquitoes do not bite birds ! Later, when MacCallum demonstrated the nature of the flagella, Bignami succeeded in finding melanin in them—*indipendentemente da Sakharoff* [38].

Of course, as soon as they discovered my Indian work in 1898 they forgot all about the *Amœba guttula*. In Chapter XVIII I described exactly some of their first very questionable doings. After that, late in 1898 and early in 1899, they claimed to have infected three more healthy men by the bites of adult mosquitoes caught at Maccarese. Koch (page 409) doubted the truth of these claims ; but I think it may be accepted because two of the cases turned out by good luck to be cases of *P. vivax*—which was about the only result the Italians obtained which I had not previously found or foreseen—and I observed the same parasite in *Anopheles* later in Sierra Leone. Then, I think, they settled down properly to the easy task of verifying by my methods the life-cycle discovered by me, in the species of mosquitoes indicated by me, the Italian " dapple-winged " ones. Possibly also they incriminated two or three Italian species of *Anopheles*, and certainly applied the Romanowsky stain to my " pigmented cells " and "germinal threads," enabling them to produce good plates showing the cytology of my cycle—a thing which I had not been able to do in consequence of the interruptions imposed upon me by the Indian authorities. The results were only what were to be expected and, including cytological results, were due chiefly to Drs. Bastianelli and Bignami. Nothing new of fundamental importance was added. In *The Lancet* of 13 January 1900 these two writers even acknowledged the value of my work on avian malaria ; but they ignored my work on human malaria which had led to it, and still could not help

introducing the usual historical subterfuges according to their custom.[1]

All this time I had engaged in no polemic with the pirates, but had contented myself with giving accurate histories of the events; and I thought I saw evidence in this last paper that their own consciences were beginning to prick them. About the middle of 1900, however, an article on malaria by E. Marchiafava and A. Bignami appeared in Volume XIX of *Twentieth Century Practice* (Sampson Low, Marston & Co., London) containing all the old deceptions; and on 8 June Manson wrote to me, "Have you seen Marchiafava and Bignami's *Twentieth Century Practice of Medicine*? Lord, they magnify themselves and belittle everybody else as much as possible. I could hardly believe my eyes when I read. The latest thing in robbery is attributable to Grassi. You recollect the filaria in mosquito's proboscis. Grassi claims that as his!!!!!!! What next?" Four days later he wrote again, "I hear from Sambon that Grassi's new book is in great part occupied with a polemic against you and Koch. Why you I don't know—unless it be that you wiped his eye."

This was, I think, the first I heard of Grassi's masterpiece [57]. According to the title-page of the original (I do not know what has happened to it since then) it was published on 4 June 1900, and was named *Studi di Uno Zoologo sulla Malaria*. It reached me some months later—a fine quarto volume of 192 pages, in large type, with four magnificent double-page plates at the end, three of them in colour, and other plates and diagrams in the text, and was published by the *Reale Accademia dei Lincei, Roma*, which corresponds with our Royal Society. The first thirty-one pages of it, besides numbers of other passages, are occupied entirely with falsifications regarding the work of Robert Koch and myself, and the remainder of it contains almost nothing, except, perhaps, one or two insignificant details, which were not known from the beginning of 1899. The object of the falsifications was to establish the two following ingenuous propositions:

"Io ho perciò determinato il secondo oste dei parassiti malarici" (page 5).

"I have therefore determined the second host of the malaria parasites."

[1] Thus in order to avoid their obligations to me, they cite the previous case of Texas Cattle Fever, carried by a tick. But the analogy does not apply, as they well knew, because a tick fed on a case of that fever transmits the disease to her own progeny and *not* directly to a healthy ox. They also give a false account of Bignami's original mosquito theory (page 339).

And—

" Giova infine far risaltare che io arrivai agli *Anopheles* malariferi indipendentemente da Ross, le cui richerche su parassiti malarici degli uccelli furono pubblicate quasi contemporaneamente alla mia prima Nota preliminare " (page 31)

" Which helps to make it evident that I arrived at the *Anopheles* malariferi independently of Ross, whose researches on the malaria parasites of birds were published almost contemporaneously with my first preliminary note."

The facts are, however, that I distinguished two species of *Anopheles malariferi* and cultivated *Plasmodium falciparum* in them in August and September 1897 [29, 30]; that Manson published my work on the malaria of birds (which proved the mosquito theory) on 18 June 1898 [33], and completed the story at the end of July [34] (*Lancet*, 20 August and *British Medical Journal*, 24 September); that Grassi's first preliminary Note [35] was not published until 1 and 2 October 1898, in two garbled versions, both of which were scientifically worthless; that Bignami did not obtain his first alleged positive result until 3 November 1898 [38]; that he, Bastianelli and Grassi did not even claim to find the pigmented cells in *Anopheles* until 25 to 28 November 1898 [40]; that some time before then they had recognised the genus of my dapple-winged mosquitoes to be *Anopheles* (page 349); and that all the work they did from beginning to end was suggested, assisted, and rendered possible by my previous work, my methods, my technique, my specimens, my descriptions, and my drawings.

Nothing more need really be said about the claims of Dr. Grassi; but I will now " reconstruct the crime " in detail in order to expose some of the artifices of the piracy. Most of the facts mentioned below will be found corroborated in a paper by G. H. F. Nuttall " On the Question of Priority with Regard to certain Discoveries upon the Ætiology of Malarial Diseases [62]," May 1901:

(1) That Bastianelli, Bignami, and Grassi were conversant with the publications of Manson and myself before they themselves obtained any definite results is clear from the letters of Charles (Chapter XVIII) and from their own writings, although, as usual, they give no complete bibliographies and no honest straightforward histories of the events. Thus they were even acquainted with my early papers in *The Indian Medical Gazette*, because they criticise my attempts to infect men up to 1896 and my experiences in the Sigur Ghat in 1897 (Chapters XI and XII).

(2) I infer that they had read Manson's important paper [33] of 18 June 1898 on my work *because I* can find no mention whatever of it in their writings; and I infer that they do not mention it *because* to have done so would have sufficed to discredit their claims at once—since the paper proved the mosquito theory, gave drawings of the pigmented cells, quoted the acceptance of Manson, Laveran, Metschnikoff, and Nuttall (page 287), and preceded even the commencement of their own efforts in 1898. Grassi, in his *Studi di Uno Zoologo*, dishonestly omits all reference to that paper. Yet his bibliography contains many references, especially to English articles, including many trifling ones.

(3) That they recognised my dapple-winged mosquitoes to be *Anopheles* is probable from Charles's letter to me of 19 November 1898 (page 343), and *certain* from their own paper [40] of 4 December 1898, in which they say: "Verisimilmente i due mosquitos colle ali macchiate nei quali il Ross in India trovè stadî di sviluppo simili a quelli del proteosoma (3° giorna circa) appartenevano pure alla specie *Anopheles claviger* Fabr." (page 349). They thought the large dapple-winged mosquito was actually the same species; in fact they had recognised it from my description of 18 December 1897, in which I had said that the former had four spots on the anterior nervure of the wing and boat-shaped eggs (page 233). Now a synonym of *A. Claviger* is *A. quadrimaculatus*, Say, so that this would be the first Italian mosquito which they would suspect, after reading my description, to carry malaria in Italy. They tried it and they succeeded—as I hoped anyone would do after reading my description. That is probably the whole history of their success, which they try to attribute to their own efforts independently of me. As Nuttall says in the paper just mentioned, my description of the "insect's eggs leaves no room for doubt." I think that they identified my mosquito early in 1898—probably before July.

(4) On page 4 of his *Studi di Uno Zoologo* (first edition—I cannot answer for later ones), and in his first preliminary Note [35] Grassi says that he commenced his work on 15 July 1898. That was nearly one month after Manson had announced my work on avian malaria in his paper of 18 June, which the Italians omit to mention. I infer that it was this paper which finally set Grassi and his colleagues to work, though probably they had already seen also my articles of 18 December 1897 [29] and of 26 February 1898 [30], announcing the pigmented cells and describing the *Anopheles* malariferi.

(5) Originally Grassi did not intend to make his discoveries independently of me, because, near the beginning of his *prima Nota preliminare* [35], as dated 29 September 1898 and published in the *Policlinico* of 1 October following, he refers unmistakably to my work and says that the mosquito theory of malaria " has acquired another strong argument in its favour since the indisputable demonstration: (1) That Texas fever is propagated by ticks, which are the intermediate hosts of the parasites of this fever, which is certainly allied to malaria. (2) That one of the so-called malaria parasites in birds has for its intermediate host a mosquito."

This latter sentence can refer only to my *Proteosoma* work; and, as he admits the work to be indisputable, he must have seen by then either Manson's two papers on it [33, 34], or my Proteosoma Report [32] itself (which Manson or others may have sent to him),[1] or, more probably, all three. In any case he knew my work when he was only beginning his, so that he certainly did not proceed independently of me.

(6) The same paper [35], with the same title (*Rapporti tra la malaria e peculiari insetti*), but undated and considerably garbled, was presented by him almost simultaneously to the R. Accademia dei Lincei on or before 2 October 1898. In that version he *had* evidently determined to proceed independently of me, because in it the sentence referring to me (just given) is entirely omitted! Compare Nuttall, op. cit. page 433. Both versions, however, indicate recognition of the law then recently found by me that infecting mosquitoes cannot be derived straight from marshes, as Bignami thought, but must be previously infected from men; but neither version mentions my name.

(7) The task which Grassi had set out on 15 July to perform was to compare the prevalence of malaria with that of various kinds of mosquitoes in a number of localities near Rome; that is, as mathematicians express it, to find the " coefficient of correlation " by means of " random sampling." No competent man of science, much less any mathematician, would dream that this could be done conclusively, if at all, under years of patient study. The guilty species of mosquitoes are often much more scarce than the innocent species, even in the most malarious localities; or they may abound there only for short periods; while the intensity of malaria in certain places is greatly affected by subsidiary causes, such as bad housing

[1] Bignami mentions the Report in *The Lancet*, 10 December 1898, page 1542, as a familiar thing [38].

or overcrowding and poverty. Such surveys can yield only speculations and not proofs. I had made one early in 1897 in the Sigur Ghat (Chapter XII) and had found a single *Anopheles* in that deadly spot! Afterwards, in 1907, at a very malarious spot in Mauritius, I found swarms of *A. mauritianus* and a very few *A. costalis;* yet it was the latter and not the former which carried the disease. *Anopheles* abound in many localities (as in England) where there is no malaria now; and A. Celli showed that even in the areas studied by Grassi there is no apparent correlation. Yet in two months the latter pretended to have found sufficient evidence to throw suspicion on no less than three species of mosquitoes by this means. Then later (*Studi di Uno Zoologo*, page 4) he had the assurance to assert that this trifling investigation not only threw suspicion on the three species, but *definitely incriminated* one of them, the *A. claviger*! Either he himself did not know the meaning of the word *proof*, or he assumed that his readers or dupes did not.

(8) What exactly did he find? His *prima Nota preliminare* [35] gave no details of his visits to malarious places such as would enable the reader to judge the value of his evidence, but, after hinting at great researches which he and Dionisi had been doing or were going to do and mentioning some obvious speculations, he goes on to decide at once the guilty species. The first species he mentions is *A. claviger*, and he notes the four spots on its wings, calls it the *vera indice, vera spia della malaria*, and says that it is found in all malarious places. This would have been a good guess (if he had not had my dapple-winged mosquitoes to guide him); but unfortunately for him he goes on to guess at two other malaria-bearing species—*Culex penicillaris* and *C. malariæ* (so called by him—really *C. vexans*, I believe). After citing a few weak arguments which do not suffice to establish even a *prima facie* case much less a verdict against any of these three species of mosquitoes, he sums up as follows in *the Policlinico version of his Note:*

"In conclusione, io sono d'avviso che il *Culex penicillaris l'Anopheles claviger* o per lo meno il *Culex pencillaris*, fors'anche il *Culex malariæ*, nella malaria si comportino come la zecca nella febbre del Texas."

"In conclusion I am of opinion that *C. penicillaris* and *A. claviger* or at least *C. penicillaris*, perhaps also *C. malariæ*, behave in malaria as the tick does in Texas fever."

Unfortunately none of these mosquitoes behaves like ticks in Texas Fever (footnote, page 401); and, out of the three

species which Grassi "convicted," two, including the alleged principal offender, *C. penicillaris*, are innocent!

(9) Turn now to the *Lincei* version of the same paper. We are amazed to find that this "conclusion" is omitted—or rather that the words "ritengo che zanzaro e zanzaroni palustri" ("I hold that gnats and marsh mosquitoes") are substituted for the actual names of the species which the author had incriminated—compare Nuttall [62, page 433]! This change quite alters the sense of the paper, which now suggests that the *A. claviger* is the most probable criminal—so that if one version were found to be wrong, the ingenious author could quote the other version! As already stated, the *Lincei* version also omits the reference to my work on avian malaria given in the *Policlinico* version.

Which version appeared first? Bignami (*Lancet*, 10 December 1898, page 1542) states that one or the other version "appeared at the end of September." The *Policlinico* version is dated at the end "Roma, 29 Settembre 1898," and the number of the *Journal* itself is dated "1° ottobre 1898." The *Lincei* version is not dated, but is published in the *Rendiconti, Volume* VII. *Fascicolo* 7°—among the "*Comunicazione pervenute all' Accademia prima del* 2 *ottobre* 1898." I infer that the garbling was done simultaneously, for the express purpose suggested above.[1]

The same Note contains another garbled passage in which the author claims vaguely to have experimented with mosquitoes fed on malarial blood, without giving details.

(10) A month later Grassi put in a second paper [36] confidently called *La malaria propagata per mezzo di peculiari insetti.* There is no more evidence in it[2]; but he repeats

[1] Most respectable Academies strictly refuse to allow the simultaneous publication of their papers elsewhere, especially in a garbled form; but the *R. Accademia dei Lincei* (of Rome) appears to have its own code of morals. . . . After my work on avian malaria it was extremely likely that the spores of *Pyrosoma* (Texas Cattle Fever) occur in the salivary glands of young ticks. Of course Grassi expected to find them there. Consequently in the index of the Atti of the *R. Accad. dei Lincei*, 1899, vol. viii. *Sem.* 1°, we find mention of a *Nota del Socio B. Grassi, Sui germi del pyrosoma nelle glandolo salivari dei giovani Rhipicephalus.* We expect yet another great discovery, and look up page 438. Alas! there we find only the above title followed by the words, "Questa Nota sarà pubblicata in un pressimo fascicolo." We are still awaiting the article, I believe. The title was evidently inserted in order to establish a claim for priority after someone else should really discover the facts.

[2] In this paper Grassi says that he has received some of my "grey mosquitoes" from Manson, and also discusses the latter's exposition of my work [34] in Edinburgh in July. But he mentions only the version in the *B.M.J.* of 24 September. The article had appeared in essentials a month earlier in *The Lancet*, 20 August—which the Italians always ignored, as they ignore Manson's article of 18 June.

(page 236) that *A. claviger*, *C. penicillaris*, and *C. malariæ* are "*enormemente sospette.*" Hence there is no doubt as to what he really did discover at that time. In the same paper he mentions seven species which he thought to be "per lo meno non necessarie," namely, five species of *Culex* and also two *Anopheles* (*bifurcatus* and *nigripes*). This was again unfortunate, since these two *Anopheles* were afterwards incriminated by the Italians themselves when they used my method—the only certain one, that of finding my "pigmented cells" or "germinal threads" in the insects. Thus out of three species which Grassi declared to be guilty, two were innocent; and out of seven species which he thought were innocent, two proved to be guilty. Yet, shortly afterwards and subsequently, he had the impudence to declare that it was these loose speculations of his which definitely discovered the "*Anopheles* malariferi" in Italy, and, by implication, throughout the world! Of course the *A. claviger* itself was not definitely convicted until the end of November [40], and then only by my method, and after its relationship to my "dapple-winged mosquitoes" was recognised (supposing that the work was genuine, page 351).

(11) So much for the "*Anopheles* malariferi." Of course in his *Studi di Uno Zoologo* [57] Grassi repeats the old falsehood about my discovery of 20 August 1897, *because it* anticipated the first Italian result by fifteen months, and, indeed, he adds six quarto pages of similar falsification to the original offence (page 351); but it is unnecessary to expose him here in detail. Dr. Edmonston Charles had evidently given him copies of my letters, for on page 11 of his *Studi* he quotes from one of them. In this way he acquired several useful hints. Thus he and his colleagues immediately "proved" that gnats of the genus *Culex* do not carry malaria; though to do so requires innumerable experiments, and it was then winter in Italy—really they took my results with "grey" and "brindled" mosquitoes on trust and pirated them. In order to cover the piracy Grassi averred that I (who discovered the pigmented cells) may have overlooked them in these kinds of gnats (compare Chapter XIV)! Early in 1899 I hinted to Charles that I had begun to doubt the nature of my "black spores"; Grassi immediately published a statement that he had disproved my conjectures on the subject—though he had done nothing worth calling science on the subject. I told Charles how the peculiar habits of *Anopheles* as regards breeding in pools and not in vessels affected the whole epidemiology of malaria; Grassi

immediately published a paper pointing out the same thing. I once looked for the "germinal threads" in the eggs of the mosquito; Grassi *almost* discovered this some months later! Many of the items in his *Studi* are directly pirated from my Sierra Leone results (Chapter XX), and I recognise one of my Indian specimens in his plates. His figure of the attitudes of *Anopheles* and *Culex* is stolen straight from me, with the usual chicanery (see *British Medical Journal*, 3 November 1900).

(12) More meretricious stuff can scarcely be imagined. Grassi discovered nothing in connection with malaria. The only Italian results of value regarding the mosquito-malaria-theory were due to Bignami, who, as he himself has said, used Grassi as an entomological assistant to identify his mosquitoes. To sum up quite strictly and accurately—it was the principal merit of Grassi to have discovered, not the "*Anopheles* malariferi," but the zoological genus of my "dapple-winged mosquitoes."

Excepting the chance discovery that *Anopheles* can carry also the mild tertian parasite, the Italian work was merely a local affair—based, like my work in Sierra Leone and that of other observers in other parts of the world, upon my Indian researches culminating in July–August 1898. Of the many species of *Anopheles* now connected with malaria only three or four were incriminated by the Italians. It is therefore an absurdity to attribute to them—as a few junior or careless writers have done—the general law connecting malaria with *Anopheles*. This law has been suggested, not ascertained, only by the efforts of many observers in many parts of the world.

On the other hand, I fear that the world will suspect Prof. B. Grassi and, to a less extent Drs. A. Bignami and G. Bastianelli, of having made a deliberate effort to pirate my work; and I think that such attempts ought to be at least criticised in the interests of honest investigators—who, unlike other injured persons, receive no protection from the law. But, having now displayed the facts, I will leave the verdict to others, and will conclude with the judgment pronounced by Robert Koch, the founder of medical bacteriology, and by Alphonse Laveran, the founder of medical protozoology, both of whom knew all the details of the controversy.

BERLIN,
KURFÜRSTENDAMM, 25,
10 *Feb.* 1901.

HOCHGEEHRTER HERR COLLEGE,

Die Deutung, welche Calandruccio meinem Verhalten gegenüber Grassi giebt, ist nicht richtig.

Nach meiner Ansicht dürfen wir einem Menschen, auch wenn er ein Schuft ist, seine moralischen Defekte nicht auf seine wissenschaftlichen Verdienste, sofern er solche besitzt, anrechnen.

Ich würde deswegen, obwohl ich Grassi für einen Schuft und einen Räuber auf wissenschaftlichen Gebiete halte, seine wissenschaftlichen Verdienste, da wo sie erwähnt werden müssen nicht verschweigen. Aber nach meiner Uberzeugung hat er keine solche Verdienste. Das, was er als solche ausgiebt, ist gestohlen oder erlogen und was dann noch übrig bleibt, ist zu unbedeutend, als dass ich mich für verpflichtet halte, es als werthvolle bereicherung der Wissenschaft zu erwähnen. Seine Angaben über die Entwicklung der Malaria-parasiten im Muckenleib sind, wenn er sie wirklich so gesehen hat, wie er augiebt (was ich übrigens nicht glaube) nur eine Bestätigung Ihrer Entdeckungen. Seine Abbildungen sind geradezu Copien der Ihrigen. Die ersten Infections-Versuche welche in Rom von Grassi und seinen Mitarbeitern gemacht wurden und mit so ausserordentlicher Reclame in alle Welt verkündigt wurden, halt ich für erfunden ; denn sie wurden in einer Jahreszeit ausgeführt, in welcher es Keine frischen Infectionen in Italien giebt.

Leider ist Grassi nicht der einzige, der in Rom auf diese Art Wissenschaft macht. Seine unmittelbaren Mitarbeiten handeln wie ich selbst erfahren habe, nicht besser. Und Marchiafava hat in früheren Zeiten Golgi und Laveran in ähnlicher weise zu beräubern versucht, wie Grassi er bei Ihnen gemacht hat. Laveran hat er in seinem Traité du paludisme 1898, p. 42. Anm. 2 geschildert.

Dieser sogen römischen Schule gegenüber muss man, wie mir scheint sehr versichtig und auch sehr skeptisch sein. Ich glaube diesen Leuten nichte mehr, als was ganz unwiderleglich und unter Zuziehung von zuverlässigen Zeugen bewiesen ist.

Mit vorzüglicher Hochachtung, ergebenst,

R. Koch.

On receipt of this letter, I sent it to Laveran, who replied as follows :

Paris,
26 *Mars.*

Mon cher Confrère,

Je vous remercie de votre letter ainsi que de la copie de celle de Koch. Je connais trop vos adversaires dans ce

débat pour m'étonner de leur manière d'agir a votre égard. Ceux qui ont suivi attentivement vos recherches savent à quoi s'en tenir au sujet des assertions de Grassi, mais c'est le petit nombre. Vous faites donc bien de vous défendre et de dévoiler les procédés mesquins et malhonnêtes de ceux des auteurs italiens qui cherchent a diminuer a leur profit l'importance de vos travaux.

Je vous prie d'agréer, mon cher confrère, l'expression de mes sentiments tout devoues.

A. Laveran.

But nevertheless something may be said for the delinquents. The thief must at least possess the virtue of energy, and the scientific thief the virtue of scientific enthusiasm. Our Roman friends possessed both these virtues—so rare amongst the Sluggards, the Do-nothings, and the Think-nots ! Great would have been their honour—if their honour had been greater.

When I first saw the *Studi di Uno Zoologo* in the summer of 1900 I laughed at it. My colleague Rubert Boyce thought it to be a bid for one of the medical Nobel Prizes—which had then been announced for annual distribution (beginning at the end of 1901) ; but I considered it to be merely paranoiacal. The only thing about the book which troubled me was that, to my amazement, and even horror, it was dedicated to Patrick Manson ! I wrote to him on the matter. He admitted that he had accepted the dedication, but at a time when he was quite ignorant of the contents ; and he took the position that a person to whom a book is dedicated cannot be responsible for all that appears in it. I remained doubtful of this as an abstract proposition, especially when the polemical tendencies of the writer of a book were as well known as Grassi's were. In fact several people, especially foreigners who did not know the circumstances, rightly or wrongly inferred that Manson had actually countenanced the attack upon me with the sponsion of his name. This was of course precisely the effect which the ingenious Zoologo had wished to produce. It was the cleverest, or rather the only clever, thing which he had done—an astute Italian trick worthy of Cæsar Borgia.

Two years later, in 1903, when he found that a Nobel Prize was not coming to him after all, Grassi produced and issued broadcast another book of 102 octavo pages called *Documenti riguardanti la storia della scoperta del modo di transmissione della malaria umana*, purporting to be a translation of important

papers on the subject (Rancati, Milano). It contains no accurate bibliography, omits Manson's publications of my work, and gives a fudged history of the events. It purports to render in full my papers of 18 December 1887 recording the original discovery of the pigmented cells; but the drawings of the cells made by Manson, and the remarks of Manson, Bland Sutton, and Thin (all of which absolutely established the genuineness of the work) are omitted without the smallest reference or explanation. Doubtless the other *documenti* are similarly garbled. This book also is dedicated to Manson! My lawyers said it was libellous, and on their advice I wrote once more to Manson about the dedication—which of course again helped to give currency to the falsehoods. This time, I am delighted to say, he was able to state that the book had been dedicated to him *without his permission!* And he proclaimed this publicly at once in *The Lancet* and *The British Medical Journal* of 28 March 1903.

When Grassi produced his *Studi* he must have forgotten that Charles's letters to me (Chapter XVIII) probably gave an accurate record of the events, though he did not scruple to make use of one of my replies without my permission. Thereupon I wrote to Dr. Charles and obtained his consent to the printing and finally to the publication of all his letters to me [61]. They were printed at the end of 1900, and were considered as published at Liverpool on 30 April 1901 (not for sale, but for circulation by me), under the name of *Letters from Rome on the New Discoveries in Malaria*. To this print I added another, dated September 1900, and issued February 1901, called *Second Postscript to the Letters from Rome on Malaria*, by R. Ross. They are both out of print now. Grassi wrote furious letters to Charles in consequence.

Apart from these pamphlets, I made several efforts to defend my work from the pirates; but the latter were for the time completely triumphant; and at least two English scientific journals refused my gentlest replies. Then I determined to carry the war into the enemy's country, and a short article by me in Italian called "Le scoperte del Prof. Grassi sulla malaria" appeared in the *Policlinico* of Rome, 1 November 1900, page 550. Grassi replied in the same for 1 December, page 593. I wrote again and Grassi replied again in the same, early in 1901, page 272. The controversy rather recalled the scholastic disputes of the Middle Ages, though the editor had excised most of my comminutive passages; but friends told me that it was found very entertaining—outside Rome.

Perhaps the most amusing passage is the following (1901, page 275). I had been discussing the third mosquito in which I had found pigmented cells in September 1897, a *Culex fatigans*, which had been *caught* biting a patient (page 236) —Grassi pretended that all my mosquitoes had been similarly *caught* feeding, and harangued me in the second person after the Italian manner! I replied:

"Grassi however now begins to apostrophise me in the style of the market place, because I ventured to suggest the age of this mosquito. 'Signor Ross,' he exclaims 'vorreste forse darci ad intendere che . . . sapete conoscere l'età delle zanzare?' My reply is, 'Yes, Signor Grassi; in certain circumstances I can guess the minimum age of a mosquito; and if you studied nature as carefully as you study the studies of others you would be able to do the same. For example, a mosquito rarely feeds until two days after it has escaped from the pupa. It then requires roughly three days (in a hot climate) to mature its eggs. Hence if it is caught when its eggs are nearly mature, and is then kept for three days more, as was the "grey mosquito," to which I refer, it must finally be at least over one week in age, just as I said the grey mosquito was when it was killed!'"

His difficulty in comprehending such a very simple matter shows how little really he knew about the insects, even nearly three years after the date at which he pretends to have discovered the "*Anopheles* malariferi." Of such is the Kingdom of the Compilers!

One reason why scientific piracy is such a disgusting evil is that a victim of it cannot easily defend himself without incurring accusations of egotism and vanity—or even of attempting to rob the robbers.[1] Generally matters can be put right only if some third person champions his cause. For two years not one of my countrymen said a word to stem the torrent of abuse to which I was subjected; but then at last Lord Lister did so. After studying the Italian claims and asking me for many papers and explanations, he incorporated the story in his final annual presidential address to the Royal Society [58] on 30 November 1900. He gave a nearly exact account of the original discovery of August-September 1897 (which the Italians so persistently falsified for their own benefit); and then, after describing my *Proteosoma* results, concluded that "Thus

[1] Thus even *The British Medical Journal* for 11 June 1904 reproved me for "taking up the position he has against the Italians." Apparently theft in science is quite a venial fault—like murder in Ireland.

was in truth established the mosquito theory of malaria." Nor did he neglect Manson, MacCallum, Koch, the Italians, and later work (see also his remarks, and others', on page 456). Next year (1901) I was elected a fellow of the Royal Society. Manson had already been elected in 1900.

But all this came too late. Enthusiasm is a tender plant, and I fear that the events of 1898, followed by those of 1900, murdered mine for medical research. I could not avoid the feeling that I had fallen among questionable things and people. Whenever I saw a microscope I imagined a figure toiling beside it in a dark hot room surrounded by legions of flies and writing, "Dear Dr. Manson, I congratulate you on your discovery," while a number of cloaked figures armed with stilettos crept in behind!

So the Great Passion died. And there is no reason why it should be revived. The Italian affair was only the beginning of a long series of attacks—from which all sanitary workers seem fated to suffer (page 487). While these foolish persons seldom get anything for their labours, many clever people can win fame, salaries, and honours by patronising, prompting, expounding, expanding, opposing, maligning, and pirating their work. Thus as regards malaria, little recognition has been given to the younger men who have really helped us, while the other gentlemen have often done very well indeed! Such is life; and no wonder that progress is slow. If men really desire solutions of their all-important medical problems they must treat less scurvily than they do those few persons who do their best to resolve them.

CHAPTER XXII

MATHEMATICS, MALARIA, YELLOW FEVER. 1900

After returning from Sierra Leone in October 1899 until the long vacation of 1900 I was fully occupied with the Report [52], with writing my little book [49] and several public lectures, and with teaching a few students. There was no material for research, though I was contemplating many investigations, because we failed in finding *Plasmodia* in birds bought in shops in Liverpool, while the conditions at the Southern Hospital were then impossible (page 375). At the end of 1899 my salary was raised to £300 a year, and Elder Dempster & Co. gave me £100 a year for advising regarding medical officers for their ships; but there was little private practice in tropical diseases in the city because officials and other people who came home from the tropics seldom resided there. I thought myself sufficiently settled, however, to take a small house, 36 Bentley Road; and in August-September 1900 we enjoyed a delightful holiday at the lovely seaside village, Llandulas, in North Wales—the homeland summer, my first real rest since 1894, excepting Kherwara! My mother was with us, and we made the acquaintance of pleasant friends, especially the Vicar, Mr. Roberts, and his three sisters, and drew health from the sea and the "long-revelling sun."

One reason why I went to Liverpool was that I hoped to find in University College there some sympathy and even help for my old work in mathematics and literature. These were my first loves; and I had originally undertaken medical research merely and purely as a duty. But by 1900, especially after the Grassi-piracy developed in June of that year, I became, as I have admitted, extremely dissatisfied with the results which had followed the performance of that duty: I had lost money over the work, I had received practically nothing but scepticism or even abuse in return, and most of my results were credited to others. Now therefore I determined to pick up again the threads of my long-neglected and almost forgotten mathematics; and in October 1901 tried to

ascertain from professional mathematicians what value, if any, my studies had possessed. It will be remembered (page 94) that I had developed a kind of vector-geometry and in 1891 had sent an article upon it to Professor P. G. Tait, of Edinburgh, who had forwarded it to an unknown friend of his, who in turn had made very light of it. I had kept no copy of that article; but I now worked up the matter again from my notes and spoke about it to Mr. F. S. Carey, Professor of Mathematics at Liverpool. If I remember aright, he told me that he himself had not worked much at vector-analysis, but that he thought there was something like my system in the recent book on Universal Algebra, by A. N. Whitehead. There, in Book VII, Chapter IV, was the whole of my system, almost with the same notation as that which I had devised! The author said that it was based upon a system invented by Grassmann many years previously. So all my time had been wasted over rediscovering a matter already well known. It was another unkind blow; but I set to work again, now with adequate literature, and compared Grassmann's system with Hamilton's quaternions once more. I concluded, and still believe—though I say so with all diffidence and respect—that the true system is not exactly Hamilton's, nor Grassmann's and my old one, but a combination of both; and I spent my leisure during five months in working out the details of it. During this period all my ardour for mathematics—much greater than my ardour even for the mosquito-theory—revived; and on 20 March 1901 I was allowed, as a humble amateur, to read a paper on the subject before the Liverpool Mathematical Society. Few of the men present were interested in this very special branch; but I published the paper at my own expense as a quarto pamphlet called *The Algebra of Space* [101]. This afterwards attracted the attention of Charles Jasper Joly, Royal Astronomer of Ireland and editor of the second edition of Sir W. R. Hamilton's *Elements of Quaternions* (1899), and I became a member of the Quaternion Society in consequence. Thus did I follow somewhat in the footsteps of my uncle, William Alexander Ross. But my studies had been frequently disturbed by the *Policlinico* polemic [59] and the *Letters from Rome* [61], until the word "malaria" made me nearly as sick as the thing itself would have done.

I must now return to this incessant topic. Some important events occurred in 1900. For months after our Sierra Leone expedition, the Colonial Office showed no signs of moving as regards West Africa beyond sending two members of the Royal

Society's Malaria Commission there, and consequently we in Liverpool determined upon another excursion. I wished to await events at home; but on 21 March the School despatched Mr. H. E. Annett, Mr. J. E. Dutton, and Dr. J. H. Elliott (the latter being two of our most enthusiastic students) to "carry the torch" into darkest Nigeria. The expedition did not return to Liverpool until 28 October, and did much and various good work on malaria, filariasis, and other subjects, which will be found fully set forth in two large quarto reports —*Memoirs III and IV of the School* (Liverpool University Press). They visited numbers of places and made a medical survey of them which ought to have been invaluable to the authorities.

I think it was in the spring of 1900 that Dr. Manson told me of an admirable "crucial experiment" which he had devised. At that time profound scepticism regarding the whole mosquito-theory still remained everywhere, and "the blue malarial mists" still occupied the brains of the doctors, if not the sites of the disease. Manson proposed (1) to infect volunteers in London by the bites of infected mosquitoes brought from Italy; and simultaneously (2) to keep healthy persons for some months in a mosquito-proof house in some intensely malarious spot in the Campagna. If these experiments succeeded the last excuse for doubt, he thought, would be removed.

The first would certainly provide a most striking confirmation of the theory. The four alleged experimental infections of healthy men in Rome in 1898–9 were not above doubt, partly for the reasons given on pages 351 and 409, and partly because Rome is surrounded by malarious areas. But the second proposed experiment would not be scientifically sound unless it were continued for an indefinitely long period. Even in the most intensely malarious spots not everyone becomes infected during a month or two, or even during a whole malaria season, or even in two or more years. Many of our soldiers escaped infection entirely during the whole Salonika campaign (1915 to 1919), though numbers of their comrades were attacked. Many planters remain well for years in very malarious plantations. It is a question of chance; infected *Anopheles* usually form only a small proportion of all the *Anopheles* in a place; and not every bullet finds its billet. Moreover the experiment would be further vitiated if the healthy subjects were to be kept in a *new* house in the Campagna—as, in fact, was done. Notoriously malaria may haunt one house and be absent from another one a hundred yards away—as we actually found in

Mauritius in 1908 (the mathematical reason for this is indicated on page 130). But very few people would think of such details ; and the double experiment would at least advertise the cause of tropical medicine and help to bring in much-needed funds to the Schools, even if it was not likely to add much to our knowledge.

Both experiments were entirely successful. A number of *A. claviger* (*maculipennis*) were fed on a case of *P. vivax* in Rome on 17 to 23 August 1900, were brought to London, and were there allowed to bite Dr. Manson's son, Mr. P. Thorburn Manson, on 29 and 31 August. He began to have fever on 13 September, and on 17 September I received the following telegram :

" Son developed tertian keeping him without quinine 48 hours can you come verify. Manson."

Unfortunately I could not leave Liverpool. The tertian parasites were duly found in Mr. Manson's blood. A second subject, Mr. Warren, was similarly bitten in London later, and the same parasites were found in him on 2 October and subsequently. Of course the facts were known already ; but a more brilliant verification of them could not have been devised. Details will be found in *The British Medical Journal*, 29 September and 6 October 1900.

Many accounts of the Campagna experiment will be found in the medical press of the latter part of 1900, especially in *The British Medical Journal*, 8 December, page 1679. Drs. Low and Sambon, and, I believe, M. Terzi, lived in a mosquito-protected hut at Ostia from 19 July to some date in November (I gather) and remained perfectly free from malaria, though that was the worst time of the year. It is impossible to estimate, from such details as are given, the actual chances of infection in the locality for unprotected persons, and therefore to gauge the scientific value of the experiment.[1] To have made it more reliable the same number of subjects should have been kept for the same period in an unprotected part of the same building—but who would have volunteered for such a duty ?

In my book, *The Prevention of Malaria*, I collected, up to 1911, no less than thirty-two more experimental infections of healthy men with *Anopheles*, carried out subsequently by Fearnside, Buchanan, Schüffner, and Jancsó in various parts of

[1] It has however been much boomed in the lay press ever since and has even been credited with having furnished the final conclusive proof of the mosquito-theory !

the world, together with fifty-one infections by means of blood taken from infected persons, done by a number of people. Some of these were conducted for useful scientific purposes; but there is not much good merely in piling up academical evidence and proving the proved. There are two kinds of proof: that which merely proves, and that which also explains —and the latter is the better. The best proof of the mosquito-theory lay in tracing the development of the parasites step by step in the insects: it explains as well as proves. In order to controvert it, the sceptic must repeat the experiments and show where they break down. After all, the disbelievers were only people who were either ignorant of the facts or who pretended to disbelieve in order to save themselves the trouble of studying them; what I call pseudosceptics. While these academical confirmations were good enough in their way, I thought that Dr. Manson and the Colonial Office were wasting their time and money in hunting very small game. It would have been much better to have set seriously to work in 1900 over the actual reduction of malaria. The carriers were known; the methods of prevention were known; and the best way to convince the public was to reduce the disease. This could be done only by Government.

I was so disappointed at the inaction that I attempted to organise a public conference on the prevention of malaria, to be held in Liverpool on 25 July, 1900. Several foreigners promised to attend, but most of my countrymen were otherwise engaged. They took great interest in the mosquito-malaria theory, but apparently none in the application of it.

About the same time I drafted and printed, for signature by a number of leading men of science, a Memorial "To the President of the Royal Society, the Right Honourable Lord Lister, M.D., LL.D., D.C.L., D.Sc., F.R.C S." After setting forth that mosquitoes carry malaria and *Filariæ*, and that "several observers of experience believe it possible to reduce their numbers materially in certain localities," the Memorial proceeded:

"For all these reasons, and especially in view of the immense amount of sickness and mortality which is daily being caused by malarial fever in many of our colonies, amongst our own countrymen, such as our merchants, our planters, our soldiers, our sailors, as well as amongst our other fellow-subjects residing there; and in view of the great expense which our colonial

governments and our merchants are forced to incur in consequence of the death, invaliding, or medical treatment of their officers and servants in those colonies; and, lastly, because of the general hindrance to civilization and commerce due to the disease in many of our most fertile possessions, we trust that some immediate action will be taken in the direction indicated by the recent investigations referred to.

"We therefore pray that your lordship and the Royal Society will, as a beginning, see fit to approach those departments of the Imperial Government which are concerned in this matter (namely, the Admiralty, the War Office, the Colonial Office, the India Office and the Foreign Office), and will invite them to issue instructions to their executive medical and sanitary officers to include the destruction of the larvæ of gnats among their ordinary sanitary duties, at least so far as the means at the disposal of these officers or of local governments and municipalities will permit, and especially in the more important towns and settlements where such a measure is most urgently called for and is most likely to be successful.

"We have the honour to be,

"Your Lordship's humble servants,

". . . . . ."

The document was signed by a number of leading men, including the Duke of Northumberland. Dr. Manson could not sign, as he was an official, but he obtained some signatures to it. Sir W. Broadbent refused (I think that he was then President of the College of Physicians), and Sir William Gowers, who had written much to me on the subject, signed rather unwillingly. Some other doctors conveniently ignored the paper and others criticised it—doctors are a timorous and therefore an ineffectual throng.

To support the Memorial I wrote the following letter urging (at least) some experiments on the subject. I knew that doctors like the word *experiment*.

A Forgotten Suggestion

To the Editors of "The Lancet"

Sirs,—The announcement of the interesting experiment to be made by Dr. Sambon and Dr. Low in the Campagna stimu-

lates me to recall attention to another enterprise in connexion with malaria suggested by me a year ago but, I regret to say, not even yet attempted. I refer to the subject of making direct experiments regarding the possibility of controlling malaria by the destruction of anopheles. It is, of course, very desirable to make as many theoretical investigations as possible on the mosquito theory, but we must not forget that while we are considering academical details valuable lives are constantly being lost and that we are already in possession of facts solid enough to form a basis for practical action. The mosquito theory scarcely stands in much need of further proof; it was established long ago. We already know that malarial fever is communicated by gnats. The next work of importance is to ascertain by systematic and thorough experiment whether gnats can or cannot be exterminated or reduced within a given selected area and what effect this measure will have on the public health. The experiment must be made sooner or later. Quinine, mosquito-nets, mosquito-proof houses are well enough in their way, but they will never be used by the bulk of those who live in malarious places. The question whether there is not also some other mode of infection is certainly one of importance, but it will find its own answer in the course of time and we have no justification for postponing action against the known cause of the disease because we are not yet quite sure that there is no other cause.

The duty of carrying out the experiments to which I refer is distinctly one which belongs to the Governments of our malarious dependencies. Private individuals possess neither the authority, the means, nor the leisure to undertake the work, and if they do so they receive little encouragement to proceed. We may perhaps be allowed to express some disappointment that the practical bearings of the mosquito theory have received so little attention in this country. I trust that the many men of influence who have already taken an interest in the theory will help to urge the advisability of shortly undertaking the obvious and necessary experiments to which I refer.

I am, sirs,

Yours faithfully,

RONALD ROSS, D.P.H., M.R.C.S.Eng.

LIVERPOOL,
May 8th, 1900.

On 21 June 1900, after receiving the Memorial, Sir Michael Foster, one of the secretaries of the Royal Society, wrote to me:

My dear Ross,

We had some discussion in the Malaria Ctee on Tuesday about urging H.M. Govt. to take practical steps towards exterminating malaria by getting ride of Anopheles. It was stated that you would be able to indicate certain definite places in India and possibly in West Africa where the local circumstances are such that a satisfactory experiment in this direction might be carried out.

Can you do this? If so will you kindly write me a short memorandum to that effect.

Your vy truly,

M. Foster.

Surgeon-Major Ross.

I have no copy of my reply. Obviously the Society had been somewhat misled by my previous letter in *The Lancet*. We did not really desire an *experiment* on the possibility of reducing mosquitoes—which would be like an experiment to prove that two and two make four; we asked the Society to urge Government to act upon a self-evident proposition. I never learnt what ultimately happened to the Memorial, but am sure that it had no practical effect whatever. It reached the Colonial Office, which—appointed a Committee!

I was still obliged to fight for the whole mosquito theory; and in *The Lancet* of 7 July 1900, page 48, will be found a long letter by me on the subject; which may amuse the reader who cares to look it up—I have no space for it. See also *The British Medical Journal*, 3 November 1900.

This book cannot possibly describe all the work done on tropical medicine during recent years; but I must mention studies connected with my own labours, and the most important of these were made by Robert Koch. I left him (on page 335) when he had succeeded in confirming my *Proteosoma* work in Italy and Germany in 1898. After that he led a German Malaria Expedition to East Africa and New Guinea. In 1901, on his return, I asked him for some details regarding his observations, and his reply will be found in my Nobel Lecture [78, page 73].

Long before this C. W. Daniels and other physicians in British Guiana had noticed, on examining the spleens of 1,289 natives who had died there, that this organ was generally much enlarged

and discoloured with the malarial pigment in *children*, but not nearly so much in *adults*; and I had even remarked in India how frequently the *Plasmodia* occurred in children. But it was left to Koch to recognise and publish the very important law which underlies these facts: most children in malarious places become infected, but when they reach puberty they tend to throw off the infection (without treatment). That is, the parasites can no longer live in them; or, in technical language, they acquire *immunity* against malaria, just as people everywhere acquire immunity against measles, scarlet fever, or smallpox. I had already guessed (pages 323, 324) that immunity against malaria may exist, but had failed to grasp the law properly. Now Koch showed that in malarious localities it is the native children who are "the reservoirs of the infection"; newly arrived Europeans become infected chiefly from them through the mosquitoes; while native adults, having already survived the illness are (comparatively) insusceptible—a most important generalisation, which was quickly confirmed by the Malaria Commission of the Royal Society and by our own expeditions in West Africa.

Unlike many medical men, however, Koch was not contented with mere microscope work, but desired to apply it at once to the saving of human life. I had recommended mosquito-reduction and nets; but he now suggested that if we could destroy the *Plasmodia* by quinine in everyone in a given locality the *Anopheles* would have no parasites to carry and would therefore become innocuous. He tried the method at Stephansort, New Guinea, in 1900, with rapid and good results; and it was soon followed up in other German possessions (see Professor C. Schilling's contribution in my book [91]) and elsewhere, especially Algeria and Italy. It is useful in a modified form where mosquito-reduction would be too expensive, but our experiences during the war clearly showed the limitations of it (page 519). A summary of the German work was published in the *Deut. Med. Wochenschrift*, 1900, No. 49.

We now turn to the researches of the Malaria Commission of the Royal Society. On page 317 I described (with some heat!) the failure of the Society to send me help in India, when it would have been of such vital importance after the mosquito-theory had been proved in the middle of 1898. But they then appointed three Commissioners; only one of whom, Dr. C. W. Daniels, was sent to me. He reached me in Calcutta just before Christmas 1898, when it was too late to complete my work with human malaria; and the other two, Drs. J. W. W.

Stephens and S. R. Christophers went to British Central Africa. In February 1899, after sending a Report to the Royal Society, Daniels left me to join his colleagues at Blantyre, which he reached on 8 April. There he wrote me numerous interesting letters. Drs. Stephens and Christophers had arrived on 1 February, but had apparently not done what Daniels, the senior member, had asked them by letter to do; and when the last-named arrived the climate was too cold on the plateau for easy mosquito-work—but he found a zygote in one *Anopheles* fairly soon. The *Anopheles* there were *A. costalis* and *A. funestus*, as in Sierra Leone; and Daniels did extensive work on their habits and breeding places; but was not able to cultivate *Plasmodium falciparum* in the latter species until the end of 1899 (in 27 out of 57)—thus finally confirming my results in Freetown (see "Reports to the Malaria Committee," Royal Society, 22 April 1901, page 41). Towards the middle of 1900, he himself suffered from blackwater fever, came home, and was deservedly appointed director of the London School of Tropical Medicine—he had been dogged by bad luck throughout. In the meantime Stephens and Christophers, after doing useful pathological work on malaria and blackwater in British Central Africa, were sent in 1898 to Sierra Leone to follow up our work there. They amplified many details regarding the habits and anatomy of mosquitoes and the epidemiology of malaria there and in other West African Colonies, and were then transferred (? in 1901) on my advice to India. The Commission was closed in 1902; after which Dr. Stephens became Walter Myers Lecturer in Tropical Medicine at Liverpool (I being Professor there), and Dr. Christophers entered the Indian Medical Service.

All this work was published by the Royal Society in eight confused series of Reports to the Malaria Committee from 6 July 1900 to 10 October 1903. Dates of movements and even of papers are seldom given. Thus the first paper of Daniels from Calcutta was not published until 6 July 1900, although it was received by the Society on 13 February 1899 and read on 16 March.[1]

[1] An extraordinary appendix is added to it under the name of Lord Lister—which he told me the zoologists on the Committee had insisted upon. In it Daniels and I are rebuked for applying the name *Coccidium* to the mosquito-stage of *Proteosoma* in birds and we are gravely warned that "It is . . . not legitimate to apply the generic term 'coccidium' to a phase of growth of another genus." But I evidently considered (provisionally) that the final stage of the parasite, found in the mosquito *was* indeed a *Coccidium* of that insect, or at least belonged to the Coccidiidæ; while the so-called *Proteosoma* was no real genus at all, being merely a name previously and erroneously applied to the young

Before proceeding to what I call the Final Fight, I must describe briefly the great discovery, or rather demonstration, which blessed the very last days of last century—a few weeks after Lister had expounded the malaria work to the Royal Society; that that scourge of tropical America, yellow fever, is also carried by mosquitoes. I need not be prolix, because the history has been already very fully given in the admirable book, *Walter Reed and Yellow Fever*, by Professor Howard A. Kelly (New York, McClure, Phillips & Co., 1906 et seq.).

As usual, as soon as the proof was given, numerous theorists who had "always said so" were exhumed by various writers. Josiah C. Nott of Mobile, Alabama, in 1848, Louis-Daniel Beauperthuy [1] in 1854, Charles Finlay of Havana in 1881, were among these (page 123). Their claims and those of the malaria-mosquito theorists are discussed in my book on the *Prevention of Malaria*. Speculations of this kind are apt to be much overrated by penmen. It is easy for persons to sit in arm-chairs and weave hypotheses; many imagined America before Columbus; but an ocean had to be traversed between the dream and the reality. Theorists who do not trouble to verify their own speculations deserve little credit; but C. Finlay was of a better order. He thought mosquitoes convey the virus mechanically from the sick to the healthy; but the experiments which he cited in evidence must have been very indecisive since this hypothesis is not correct. Living as he did in the centre of yellow fever, however, he was able to form some conjecture as to the species of mosquito involved—a pot-breeding brindled one called *Stegomyia fasciata* or *calopus*, very common in Havana.

Early in 1900 yellow fever appeared among the American troops in Havana. Surgeon-General Sternberg, who was a straight thinker, immediately appointed a worthy commission to investigate it, consisting of Dr. Walter Reed, Dr. James Carroll, Dr. Jesse W. Lazear, and Dr. Aristides Agramonte. They commenced work at Havana in June 1900 and speedily

avian stages of the said *Coccidium* of mosquitoes—just as the young of eels (and of many kinds of parasites) were formerly thought to be true zoological species quite distinct from the adults. See footnote, p. 297.

[1] R. Boyce and R. Blanchard have specially advocated Beauperthuy, but they were both very uncritical writers. R. Blanchard even made the mistake of supposing that Laveran had been preceded by a writer who had really mistaken the ordinary "artifacts" found in blood for parasites! Beauperthuy really seemed to think that malaria and yellow fever are due to the salivary venom of certain kinds of mosquitoes, like the symptoms produced by snake-bite. His proofs were of the order of the Grassian proofs of the *Anopheles* malariferi.

demolished the *Bacillus icteroides* of Sanarelli as the cause of the disease. Next they determined to investigate by direct experiment the carrying capacities of mosquitoes—on the basis, not of Finlay's ideas, but of my work on malaria, according to which the virus must develop for some days within the carrying mosquito. This view was strengthened by some previous work of that most sagacious observer, Dr. H. R. Carter (to whom my highest respects), who had actually proved that an interval of about twelve days must elapse between the arrival of a case in a locality and the infection of the first secondary case there—we now know that this interval is spent in the development of the germ within the carrier. But the Commission began the work with Finlay's mosquitoes, hatched from eggs which he actually gave them. On 27 August 1900 Carroll allowed himself to be bitten by a *Stegomyia* which had been fed twelve days previously on a severe case of yellow fever. He was taken ill on 31 August with typical yellow fever and nearly died. A second similar case, X. Y., also became infected; but nine men who had been bitten by *Stegomyiæ* which had not been kept for twelve days after their original feeds remained well. While Carroll was ill, Lazear, who happened to be working in the yellow fever ward, was accidentally bitten on the hand by a "wild" *Stegomyia* in the ward. If I remember rightly Dr. Lyster told me at Panama in 1904 that he was present and that Lazear remarked, "I wonder whether this creature is infected," and allowed it to go on feeding. Lazear was taken ill a few days later and died on 18 September. But the Commission was not yet satisfied. They built a camp, called Camp Lazear, at which stringent crucial experiments were conducted. It was ready on 20 November, was strictly quarantined, and contained two "frame buildings each 14 × 20 feet in size." In one of these, plucky young American soldiers who volunteered for the purpose were kept for twenty nights exposed to the horrible soiled clothing and bedding of yellow-fever cases—without a single one of them becoming infected. In the other, Private John R. Kissinger (who said he volunteered "solely in the interests of humanity and the cause of science") was bitten by five infected mosquitoes on 5 December. He became ill on 8 December. Next week, three of the Americans who had previously been exposed to the soiled clothing were now bitten by infected *Stegomyiæ* and were subsequently attacked with yellow fever. Lastly, John P. Moran was bitten in the chamber on 21 December and was taken ill on Christmas morning. On

the last day of the dying century, Reed, the leader of the Commission, wrote home to his wife,

COLUMBIA BARRACKS, QUEMADOS, CUBA,
11.50 p.m., *Dec.* 31, 1900.

Only ten minutes of the old century remain. Here have I been sitting, reading that most wonderful book, *La Roche on Yellow Fever*, written in 1853. Forty seven years later it has been permitted to me and my assistants to lift the impenetrable veil that has surrounded the causation of this most wonderful, dreadful pest of humanity and to put it on a rational and scientific basis. . . . The prayer that has been mine for twenty years, that I might be permitted in some way or at some time to do something to alleviate human suffering has been granted !

So. He died, rather suddenly, on 22 November 1902 ; and the wealthy American people allowed him to do so without any adequate honours or reward, and actually in a state of apprehension regarding the future of his wife and daughter. This was not only a case of ingratitude, but one of impolicy. If the world refuses to pay for world-service while it allows anyone to enrich himself by self-service, well, it is the world that suffers—for its own folly. Diseases which are probably easily curable or preventable are slaying us by millions to-day, and yet we scarcely trouble even to thank the men who show us how to combat them ; but we call ourselves civilised ! . . . Fortunately his friends were able to raise a small commemoration fund for Reed and, I understand, gave Mrs. Reed the interest of it during her life.

If I were a millionaire I should give my money, not to institutions, academies, and universities, but to men like Reed, in order to make them independent for life. Those who have actually won decisive scientific victories in the past know best how to win similar victories in the future. But they must be guarded against the scientific middleman, the managing committee, and the educational company-promoters, who are often to-day the parasites and exploiters of talent. They should be free to work where and how they will.

CHAPTER XXIII

THE DASH FOR VICTORY. WEST AFRICA 1901

THE American discovery was fully announced in February 1901 at the meeting of the Pan-American Medical Congress at Havana (*Jour. Amer. Med. Assoc.* 16 Feb. 1901, page 431). Although the actual germ of yellow fever still remained undiscovered, yet our ignorance regarding its mode of propagation had been overcome in six short months. This was due to the concurrence of various circumstances: the intellectual capacity of Sternberg and of his Commission; the presence of the public-spirited American soldiers among many cases of the disease; the mosquitoes of Finlay, the "extrinsic incubation period" of Carter, and my previous findings regarding malaria. The Americans have never done greater work. The "case-mortality" from yellow fever is much larger than from malaria —about one case in four dies, while almost every newcomer in Havana was almost sure to get the disease sooner or later. In 1900 we in Liverpool had sent Drs. Walter Myers and H. E. Durham to Brazil to study it; both contracted it almost at once, and Myers died of it. Now all this was to be changed.

In the meantime, in spite of my preoccupation with the "algebra of space," my blood was beginning to boil over with wrath—I admit it without repentance—at the inaction of the British authorities regarding the prevention of malaria; and before the American news reached us early in 1901 I was preparing another plan of attack which some thought Quixotic and which has certainly never been tried before or since.

So far as I could learn, nothing at all had been attempted. My letter of 16 February 1899 to the Director-General of the Indian Medical Service, outlining my method of malaria-reduction, had been published in *The Indian Medical Gazette* for July 1899 [45], but had met with absolutely no response in that Empire except a letter from Captain S. P. James, I.M.S., in the same periodical for November 1899 in which he said that the "dapple-winged mosquitoes" were to be found in rice-fields round his bungalow in Travancore. In some way

he seemed to think that this discovery invalidated my method; and I heard subsequently that the Indian sanitary authorities, always naturally slow to move, had seized upon his welcome paper as a further pretext to do nothing. About the same time G. M. Giles studied Indian mosquitoes again (page 230); but his papers and book [54] showed little appreciation of the practical sanitary side of the subject. My friend, Colonel W. G. King, I.M.S., Sanitary Commissioner of Madras (page 212), was the first upon whom such ideas dawned, and on 31 May 1900 he wrote to me: "I have started all Municipalities on anopheles hunting expeditions!" Dr. Nield Cooke, Health Officer of Calcutta, made some preliminary enquiries (*Lancet*, 8 September 1900), and he and Captain L. Rogers, I.M.S., put tar, on no less than two occasions, into a ditch which was causing malaria in some "coolies' lines" in 1900; but they seemed to be disappointed that the fever did not immediately vanish (*Indian Medical Gazette*, November 1900). In fact, after such vigorous experiments, my ideas were already becoming quite discredited in India; I was not popular there, and was accused of holding many foolish opinions—so much so that I was forced to protest as follows in *The Indian Medical Gazette* for December 1900:

"The habit of imputing to a writer opinions which he has never expressed and has indeed often disclaimed, and of then demonstrating simultaneously the folly of these opinions and of the writer for holding them, is one to be guarded against. I have really never expressed the 'ingenious suggestions,' which Captain * * * seems to think I have, regarding the possibility of exterminating anopheles from, let us say, the whole of Bengal! The utmost I ventured to suggest was that it might be possible to exterminate them from *some* large towns, cantonments, and plantations, *under favourable conditions*. So I think it is; but I have always expressly excluded *large rural areas* from this suggestion. The idea that vast tracts, peopled only with natives, can be freed from any mosquitoes is too silly even to require a disclaimer."

I quote this old matter now because the same absurdities are still imputed to me. Neither then nor for years afterwards could I find in Indian writings the smallest intelligent understanding of my proposals, much less any serious effort to apply them.

Exactly the same thing happened in Sierra Leone after we had left it in September 1899 (page 388). On 22 September I had sent a report to the Governor, at his request, with a map of the breeding-pools of *Anopheles* in Freetown, and an outline of my proposals for that town. I advised mosquito-reduction by drainage, by oil, and by hand; wire-gauze to windows, especially of hospitals and other public buildings; bed-nets for patients and, if possible, for everyone; and better houses and segregation for Europeans. But I was obliged to be careful, because it was obvious that the authorities would scarcely consent to spend any adequate sum for sanitation, though money was being poured out for invaliding, pensions, leave-pay, and other expenses caused by the sickness. Hence while I said frankly that "the most permanent and satisfactory measure is the obliteration of the breeding pools of *Anopheles* by drainage," I knew that the funds for this would not be obtained without much wrangling and for a long time, and I wished to lead on the Colony gradually to undertake temporary measures meanwhile, such as oiling and brooming the pools. This would have required a score or two of workmen under supervision, costing altogether about £500 a year or more. But as it was evident the Colony would not face this expenditure (equal to that of a single junior European official), I determined to begin by advising the employment of one native *moustiquier* (mosquito-man) to do what he could—hoping that as my ideas began to soak into the Colonial mind, he would be given more and more assistance until the temporary work was adequately done and the general drainage scheme was commenced. What happened? I understand that the single native was employed for some time, at a salary of one pound a month; but, as malaria still continued, the municipality finally discharged him and decided to give £500 a year instead to one of the Government medical officers to do duty also as Health Officer of Freetown! I gather that he was allowed no additional funds for an adequate labour force—and certainly the town was in a truly deplorable condition when we returned in 1901; but for this work he was finally and suitably rewarded.

From accounts which were given to us in Liverpool by our later expeditions, by clerks, and by others returning from Sierra Leone, we learnt that absolutely nothing was being done there during 1900 in the practical sanitary line; but, as I said on page 423, two members of the Royal Society's commission investigated the mosquitoes in the Colony at that time. These gentlemen were able junior pathologists, but, I understand,

had little acquaintance with practical sanitation; and, though they made observations of interest, they scarcely helped our cause. Thus they seemed to think that I was convicted of some serious error when they found *Anopheles* larvæ in the drying-up streams during the hot weather—where else would they expect to find them? And they laboriously proved by experiment that fresh crops of larvæ appeared in pools in which previous crops had been destroyed by oiling—as if anyone had ever dreamed that anything else was likely to occur. All this was set forth in a long and unfortunate manner which confirmed the local authorities in their evident intention not to trouble themselves at all about the matter.

In fact towards the end of 1900 it became obvious that I was in for a third battle. The first was over Laveran's parasite; the second was over the mosquito-theory; the third was over the question whether we could really reduce mosquitoes at all! On 28 November 1900 I lectured before the Society of Arts, analysed the various possible methods of preventing malaria, and appealed vigorously for mosquito-reduction in towns. No good! The chairman, a distinguished zoologist (I would not reveal his name for worlds!), summed up against me, and though his forcible remarks were not fully printed he certainly said, if I remember right, that mosquitoes are everywhere; they surround us like the air we breathe; if we destroy them in one place they will rush in from outside—and so on. He seemed to think that mosquitoes constitute a kind of gas, which instantly fills up a vacuum! There were doctors to right of me, doctors to left of me, who laughed at the mere notion of reducing mosquitoes. Some even said it was wicked—like my friend mentioned on page 58. My colleague Boyce (who afterwards wrote *Mosquito or Man*) thought I had gone too far. Mr. Jones, our chairman at Liverpool, changed the subject when I mentioned it. Sir Michael Foster asked my advice regarding the prevention of malaria, for use in a pamphlet which he was preparing for the Malaria Committee of the Royal Society; but when it appeared (under the names of Lord Lister and himself) my suggestions regarding mosquito-reduction were omitted.

At the same time all kinds of ingenious devices were recommended by various people, some of whom had never even been in the tropics. The most popular measures were to leave the mosquitoes to swarm everywhere as before, but (1) to give quinine to everyone as suggested by Koch (page 422), or (2) to protect all dwellings by wire-gauze. Imagine these measures

applied to a large town such as Freetown containing 30,000 negroes, including some thousands of infected children and many large and pragmatic mothers. It would require a regiment of doctors and dispensers to administer the quinine, a regiment of soldiers to keep order, and a regiment of newspaper editors at home to overcome the political consequences which would follow—imagine the squalling of the children, the fury of the mothers, the angry silence of the fathers, and the lofty protests of the politicians. Then to protect all the houses—wooden thatched shanties with holes and chinks everywhere in floors, walls, and roofs—we would have to convert each into a kind of meat-safe, costing much more than the original dwelling—a city of meat-safes! At the same time, out in the open, people were to wear veils, gloves, and "mosquito-boots" in the hottest weather (especially if they were soldiers marching to battle!)—though, in many places in the tropics, mosquitoes seldom bite in the open. Other experts recommended everyone to keep his skin smeared with "culicifuges," that is, lotions or ointments supposed to repel the insects. But a few *did* support mosquito-reduction—only not by draining or oiling breeding-waters, as I recommended. One person invented a lamp to attract and destroy thousands of the adult mosquitoes every night—a most ingenious invention, though unfortunately mosquitoes are not attracted by light. Others said that a loud humming note of a certain pitch would draw in all the male mosquitoes from far and wide, and a certain great inventor set to work to devise a suitable machine for this purpose (there may be something in this). Others again were convinced that certain plants, if kept in bedrooms and verandas, would repel the enemies of mankind—and so on. On the other hand, mosquito-reduction, though it is really only a refinement of the very ancient and well-approved method of banishing malaria (and is indeed applied every year at certain seasons by Nature herself), was derided by most of the scientists.

It is worth asking why. There is the scientist, with eyes to see but no brain to think, and the scientist with brain to think but no eyes to see; and the combination is rare. The biologist often does not possess the calculative faculty—which Plato rightly decreed was essential to those who were to be admitted into his Academy. The doctor and the zoologist are trained rather in observation than in calculation; the one thinks in terms of medicine and the other in terms of classification and bionomics. The doctor is often entirely ignorant of practical sanitation; he stands aghast before a few roadside puddles

and the problems of town-management, but delights in the idea of pouring quinine down everyone's throats for ever—especially if, as happens in certain foreign countries, he dispenses his own cures. The entomologist is busy over wing-veins and the pathologist over parasites; and application to life-saving is apt to be of secondary interest to them. On the other hand, this is the sanitarian's principal motive, and he must be made up of calculation; he deals with men in the mass; he fits his measures to his means; and his great science of epidemiology should be largely a branch of applied mathematics. The question which is the best method for dealing with malaria in any place or country is a sanitary question, to which medicine, parasitology, and entomology are ancillary. As I showed later in my mathematical studies [87, 91], there are no less than *fourteen* independent factors or variables which decide the prevalence of malaria anywhere. Different measures may therefore be required in different circumstances. Screening is useful for isolated dwellings, quinine where other measures are not or cannot be used, and combined measures elsewhere. But for crowded areas by far the most economical measure now known to us is constant mosquito-reduction (not necessarily extermination), practised as a part of ordinary sanitary work—not only for a month or a year, but for ever. It costs money, of course; but usually less than the sickness costs, and far less than the other measures wrongly applied. The matter is fully discussed in my *Prevention of Malaria* [91] and in Malcolm Watson's *Prevention of Malaria in the Federated Malay States* [99].

As to the possibility of reducing mosquitoes, that was self-evident. They are not uniformly diffused like a gas, but vary greatly in numbers from spot to spot. For all we know each may be *able* to fly a hundred miles during its life; but actually they tend, like other animals, to congregate where the conditions are most favourable. It is for us to render the conditions unfavourable. The amount of malaria in a place must be a function of the number of carrying *Anopheles* there, and we can at least reduce both if not banish both. *Ceteris paribus*, the *per capita* cost of this must vary inversely as the density of the human population—that is, be least in towns. Mosquitoes are vermin. To reduce vermin one destroys them and their breeding places as fast as possible. One does not stand about and think over it.

This, however, is exactly what the British had been doing for years. It was no use arguing with them, because few of

the glibbest talkers ever read what one wrote, or even knew the meaning of the mathematical word *function*. But, as I learnt subsequently, there were other reasons. One gets much more *kudos* by writing scientific papers than by doing humdrum sanitary work, however useful; and governments prefer spending money on more visible things, such as new buildings, additional secretaries, and hinterland campaigns. The Americans showed much greater intelligence. As soon as they proved in Havana at the end of 1900 that yellow fever is carried by mosquitoes, they set to work without a moment's talk and cut off the mosquito-supply of that city in a few months. The yellow fever disappeared as if by magic; and, exactly as I had predicted, the malaria-rate was soon greatly reduced.

But early in 1901 I knew nothing of this, and therefore determined on the temerarious and Quixotic action mentioned above. I proposed, in short, to raise subscriptions in England, to go to Freetown with an assistant, to hire labourers there, to buy pickaxes, shovels, and oil, and to show them how to reduce mosquitoes myself, or die in the attempt! It was really an unheard-of piece of audacity—I was going to take over the (unused) functions of the British Government. It was sanitary rebellion; it was sanitary Bolshevism. I proposed to supersede His Majesty's lawfully constituted Sanitary Department of Sierra Leone; to "wipe the eye" of the Governor and Council; to kill his mosquitoes under the very nose of the newly appointed Health Officer, with his additional salary of £500 a year and no staff! *And we did it.*

While I was mooting this wild project to men of business in Liverpool who were genuinely interested in West Africa, news came (*Lancet*, 12 January 1901) that Dr. J. C. Thomson, of the Medical Department of Hong-Kong, had been investigating mosquitoes [1] there with a view to their reduction; and I also heard from Dr. H. Strachan, of Lagos (page 379), that Sir William MacGregor, the Governor, was proposing to drain large areas of marsh and to enforce general quinine-prophylaxis among the Europeans in that town. The American news must have reached us about March; but that regarding the mosquito-reduction work of Gorgas at Havana could scarcely have arrived until some months later, as it was not begun till February (page 453). Meanwhile our African merchants were becoming less and less pleased with African sanitation, or rather insanitation, and their dissatisfaction culminated in a deputation to Mr. Joseph Chamberlain in March 1901.

[1] This he began in May 1900—not 1901—as wrongly stated in my book [91.]

It was not possible to commend either the Colonial Office or the West African governments for their vigour. At the opening dinner of the London School of Tropical Medicine on 10 May 1899, Mr Chamberlain had said, regarding malaria: "The man who shall successfully grapple with this foe to humanity, and find a cure for the fevers depleting our colonies and dependencies in many tropical countries, and shall make the tropics livable for white men, who shall reduce the risk of disease to something like the ordinary average, that man will do more for the world, more for the British Empire, than the man who adds a new province to the wide dominions of the Queen." Well, this was just what we had shown them how to do—and the speaker was just the man who had failed to do it. They had ignored the advice, not only of our expedition of 1899, but of that of 1900 (page 416). They had not even evinced any sign of gratitude for what we had given them in demonstrating the cause of African malaria and how to reduce it. Thus the Colonial Office refused official help and recognition to the Liverpool School until July 1900, long after it had granted these to the London School, so that several papers, including the Medical Press (4 October 1899), had remarked upon the point; and though it sent curt and formal thanks to the chairman of our School for our reports, not one of the actual members of our expeditions, some of whom had spent seven months in dangerous parts of Africa on a third the salaries given to Government medical officers, ever received the smallest acknowledgment for his wholly private and voluntary services—a miserable *gaucherie*. So far as I remember, not one of us was ever consulted regarding our experiences and recommendations, nor asked within the doors of the Colonial Office; and the subscribers to our funds were equally ignored. But much worse than this, nothing was done to give West Africa a proper sanitary service, such as the new discoveries demanded.

I have lost some of my papers referring to that time; but early in March our School was asked to discuss the matter in Liverpool with the Chambers of Commerce of Liverpool and Manchester. I drew up a programme of the principal sanitary reforms required for West Africa. It included elementary items of town-management, proper sanitary regulations, and, above all, a sanitary commissioner to inspect regularly down the coast and report to the Colonial Office—an absolute essential. It was decided to forward these recommendations to the Colonial Office and to ask Mr. Chamberlain to receive a depu-

tation on the subject. He agreed, and saw us at noon on 15 March 1901.

This was my first, but by no means my last, experience of such events. They remind me of a clever picture which I possess by Mr. Gordon Browne, R.I., called "A Deputation to Circe." The poor sailors to whom that beautiful but cruel enchantress has given the grotesque heads of various animals stand trembling before her on the sea-sand of her island evidently beseeching her to restore them, while she sits aloof and aloft upon a rock laughing at them! So with modern deputations: most of the brief time allotted is wasted in preliminary complimentary speeches; those who know the business are then allowed a few minutes each; and, lastly, the Minister (who is always on his defence) easily scatters the experts with a few Pythian shafts feathered with the *argumentum ad ignorantiam:* and the deputation files out again, glad to have any heads at all left on their shoulders. I think that on that fateful morning some of us breakfasted with Alfred Jones in a club in Northumberland Avenue. Next we had a preliminary meeting to discuss our procedure, at which we were joined by Mr. F. Swanzy, who represented the London Chamber of Commerce. Mr. W. F. Lawrence, M.P., introduced us; and there, in a large dark room in Whitehall, was the great-little Joseph Chamberlain, seated before us with his political trade-marks, the orchid and eyeglass, complete—a familiar type, acute, but not penetrating, sharp but not sure, deft but not deep, straight but clever. I summed him up as a man of display rather than of possession, and not very capable of any profound analysis or integration. He was accompanied by some of his officials, I have forgotten whom (Manson was not present); and after the introduction Jones, who was then, I think, President of the Liverpool Chamber, followed with a speech designed to assuage in anticipation the Minister's wrath by quoting his own words (given above). The Manchester representative then wasted much time over a local sanitary defect near his business place in Lagos; good Mr. Swanzy spoke briefly for London; our Dean, Boyce, reiterated our recommendations; and Dr. Carter, of the Southern Hospital at Liverpool, wasted more time. Next came the turn of us three scientific helots, whose combined salaries scarcely amounted to £600 a year [1]; someone looked at his watch and the reporters yawned. I compared West African sanitary organisation with that at home and in India, and insisted upon

[1] Fielding-Ould had none.

what every sanitarian of the smallest experience knows to be fundamental necessities—sanitary commissioners to inspect, advise, and report—like those employed in India and like the Inspectors of the Local Government Board in England—without which local measures are apt to degenerate into zero. Fielding-Ould gave some of his actual experiences; and Annett said, regarding Freetown, "I must state that on my second visit to that town last year it was with extreme disappointment that I perceived no indications whatever that the slightest effort was being made to improve the wretched conditions there, either in the direction of suggestions of the Expedition or in any other way." Regarding Nigeria, he added: "I urge that in these Nigerian stations, with their small European communities, a few inexpensive measures, intelligently executed now, would render those stations absolutely free from malaria." Then Chamberlain rose and demolished us—while the reporters scribbled as fast as they could. He rejected our proposal regarding sanitary commissioners, by whom he seemed to think we meant back-yard sanitary inspectors! "If," he said, "we are to have anything equivalent to a house-to-house inspection in these colonies in the West Coast of natives as well as of Europeans; if the inspectors in turn are to be inspected by a superior chief or head, with all the scientific requirements which are necessary; and if, again, these inspectors are to be supervised by travelling inspectors and commissioners sent from this country—I confess I tremble at the budget which will be produced." [1] Neither he nor his officials had understood in the least what we meant. As usual with politicians, he deprecated expenditure, not recognising that sanitary expenditure is an insurance against the much greater expenditure caused by sickness, as that on fire-engines is against fires. On the other hand, he was "prepared to consider" a travelling commission of three business men and one scientific expert, all of whom would have to be paid by the Chambers of Commerce for doing the business of the Colonial Office. This proposal was characteristic of British administration: instead of doing cheap and necessary work it spends large sums on expensive and worthless talk. The proceedings now closed with more compliments. Chamberlain had done some good (and won much political capital) by suggesting the schools of tropical

[1] From a full report of the proceedings published by the Liverpool Chamber of Commerce in 1901. Our proposals would not have cost nearly as much as his own salary or that of one of his West African governors. He did not mention mosquito-reduction, and had probably never read one of my papers, though our first Report was dedicated to him (see also page 488).

medicine (page 367); but, in my opinion, his refusal of a proper sanitary organisation for the colonies largely cancelled, then and since, the benefits which might have accrued. I suppose that I was the only one present who had any real knowledge of tropical sanitation; and I remember thinking to myself angrily as I left stately Whitehall: "These people are no longer fit to hold the hegemony of the world." Probably the fault lay with the permanent officials; but in either case my dreams of general British action against malaria (page 389) vanished at that interview. Jones and Boyce attributed everything to jealousy.

Of course the proposed commission came to nothing. Business men were not likely to undertake such a job. The Liverpool Chamber recommended me (without my consent, I believe) to be the "scientific expert"; but the Colonial Office replied on 11 July 1901:

"Mr. Chamberlain does not know whether the London and Manchester Chambers have concurred in the proposal that Dr. Ross should be the scientific expert; but he desires me to observe that, as Dr. Ross has just started for West Africa on another [*sic*] mission, it will hardly be possible for him to undertake the duties which it is proposed that the scientific expert on this Commission should discharge, and I am to add that, while Mr. Chamberlain fully appreciates the great value of the work which Dr. Ross has done and is doing in his own line, it appears to him that the expert of the Commission ought to be a Sanitary Engineer, especially as one very important part of the work of the Commission will be to report upon the cost of any reforms suggested and the means of meeting this cost by taxation."

I suppose he thought that I had suggested a sanitary commissioner for West Africa in order to find a job for myself! At that time I could have returned to India with a large salary whenever I pleased. Neither appointment was possible for me, because my capacity for continued service in the tropics was exhausted.

But I was ready to pay short visits there; and now fortune favoured us again. Something I had said about my project described on page 433 bore fruit, and early in April I received an inquiry from Mr. James Coats, junior, of Glasgow, regarding the scheme. I described two schemes, a small and a large one

—the latter being the one I have outlined ; and I then received the following generous letter :

PAISLEY,
30 *Apl.* 1901.

DEAR SIR,

I am in receipt of your letter of the 29th inst., and have read it carefully. I am willing to spend one thousand pounds stg. in the manner you describe in your *Big Scheme*, as the subject therein referred to is such a *very* important one ; and from your intimate knowledge of the habits of the Malarial Mosquito, would be likely, under your charge, to be treated in such a manner as to deserve success, and let us hope to secure it. The sum stated should permit of a Year's Trial of your Plan, sufficient to test it well.

I do not wish my name brought prominently into notice, and the arrangements can be carried out, if you like, under the auspices of the Liverpool School of Tropical Medicine. I will send you at once what part of the total sum you now require, and the balance at such intervals as you may mention.

A reply at your convenience will oblige.

Yours truly,
JAS. COATS, JR.

My pleasure may be imagined. Mr. Coats has long been dead and, though I did not divulge his name during his lifetime, I think that it should now be made known, if only as an acknowledgment of his generosity, which is due to his memory. He doubled the amount by telegram when we were upon the point of departure.

I spread the good news everywhere and at once prepared for the expedition, which I decided should reach Sierra Leone in June, before the onset of the rains and of the malaria season. This left me only six weeks to make all arrangements. My first care was to find a reliable assistant, whom I could leave to carry on the work on the Coast after my return to my teaching duties in Liverpool. The post was advertised ; and I finally selected, among several candidates, a young Scotsman, Mr. Logan Taylor, M.B., B.S., of the pathological laboratory of Glasgow University, whose ability and devotion to work cannot be too highly praised ; his salary was to be at the rate of £500 a year. In order to obtain complete freedom of action I determined to keep Mr. Coats's money under my own

control to begin with and to hand it over to the School after my return; and for this purpose I opened an account, which I called later the Tropical Sanitation Fund, at the Bank of British West Africa. Of course I could not take anything out of this for myself, either for salary, allowance, outfit, or even expenses (except cost of a passage from Lagos to Accra); and my poor pocket was again further depleted very considerably in consequence. But Mr. Coats's generosity enabled me to enlarge our scope, and I determined to extend my own visit to the Gambia, the Gold Coast, and Lagos. At the same time help came in from various kind and even enthusiastic friends. Mr. Jones promised us free passages anywhere in his steamers; Mr. Max Muspratt gave us many barrels of cement for filling in mosquito-pools; Mr. F. Swanzy added something else, I believe; and Mr. John Holt, who became afterwards a most revered friend of mine and donor of the Holt Medal to the School, presented us with numbers of barrels of crude petroleum and creosote. I think also that we received gifts of pickaxes, shovels, and watering-pots for spraying oil. Still further, as Surgeon-General Harvey, D.G., Indian Medical Service, was again at home, I wrote and asked him if the India Office would like to send one of its officers with me to learn the work. He immediately agreed (26 May), and Lieut. A. McKendrick, I.M.S., came to us for this purpose for one month in Freetown. On the other hand, the Colonial Office refused to do the like with Dr. G. Williamson, one of its medical officers in Cyprus, who wished to come and had taken a course under me at Liverpool, because, they said, "so far as we can make out, Dr. Williamson has had no experience of malaria, as the disease is practically non-existent in Cyprus." They had only to look at their own reports to ascertain the contrary, and in 1913 they were obliged to send me to Cyprus to deal with the malaria there. Countries conducted in this manner can scarcely prosper.

Of course we were obliged to ask Mr. Chamberlain's consent to our adventure; and, somewhat to my surprise, he not only consented but gave us his blessing. In fact I hoped that, though the authorities refused official war against mosquitoes, yet the European colonists would follow our example and take it up for themselves. But I was careful to explain to all and sundry that we proposed *only to give an object lesson in mosquito-reduction*: we could not continue the work indefinitely, nor did we expect to banish malaria from the whole continent once and for all at the cost of a thousand pounds! Yet this is

precisely what we were afterwards accused by the West African authorities, especially by the doctors, of attempting.

Though I did not know it till later, mosquito-reduction was being done or commenced at the same time in other places besides Havana, Hong-Kong, and Lagos, especially by Drs. W. N. Berkeley and A. H. Doty round New York, and by Dr. Malcolm Watson in the Federated Malay States—the beginning of a magnificent campaign [91, 99]. There had also been an interesting outbreak of malaria in Holland, dealt with later by turning the sea into the canals (*British Medical Journal*, 26 January 1901).

Before leaving England I received two honours. On 11 April I was elected a Fellow of the Royal College of Surgeons without examination (luckily), as I had been a member for twenty years; and I was elected a Fellow of the Royal Society on 7 June. I received many delightful congratulations, and a dinner from our University Club in May. On 6 June Alfred Jones gave us all a grand banquet, partly in congratulation to me and partly as a farewell before our departure. The Lord Mayor and the Bishop were present, and Surgeon-General Harvey came all the way from London to attend it. There were handsome speeches—which I fear I have forgotten—and we were all very proud of ourselves. I was particularly so, because during the last few months I had written my little book on malaria for laymen [66] (page 391), had completed my *Algebra of Space* (page 415), had printed Charles's letters and quashed Grassi in the *Policlinico* (page 411), and had now organised this expedition. But it had been impossible to visit Mr. Coats at Ferguslie House, Paisley, because he was away yachting.

So on 15 June 1901 we went on board the s.s. *Axim*, with barrels of cement and oil, pickaxes and shovels, insecticides, oil-squirts, etc., complete. Many friends saw us off, and others, particularly my old friend, Dr. H. E. Haycock, of Bartholomew's Hospital, then practising at Alfreton, Derbyshire, wrote to us. My mother came to stay with my family at 36 Bentley Road during my absence, and, as I have said, Mr. Coats sent us another thousand pounds on starting. We reached Teneriffe on 21 June, and I wrote to my wife:

"We had a very pleasant day there. Taylor, three passengers and I went ashore at Santa Cruz and then went up the hill in an electric tram like the ones in Liverpool only smaller. Arrived there, we had lunch at the hotel of Laguna.

After lunch we got on to five donkeys for a long ride. You can imagine that the sight was amusing. Most of the donkeys were scarcely bigger than goats and all had the most extraordinary saddles. Here is a picture of mine ! It had no stirrups and there was no bridle except a rope. Of course we were followed by a swarm of donkey boys and men who drove us forward. The population turned out to see and cheered when the donkeys broke into a 'gallop.' . . . We went along the side of some hills and then through a forest of juniper and laurel (or plants like these) and at last emerged on the top of a hill. Here we had the most beautiful views. First, there was the peak of Teneriffe, one of the most beautiful mountains, twelve thousand feet high. Then below there was a rugged valley breaking three thousand feet down to the sea, and lastly the sea and Santa Cruz on the other side. We then scrambled back to Laguna and took the tram down again, had dinner at an hotel and went on board."

After touching at Grand Canary, we arrived at Bathurst, the capital of British Gambia, on 28 June—a small town on a sandbank in the middle of the river. The heat was terrific, as the rains had not yet broken here. Our ship was delayed two days, being tied up to the wharf, and we were devoured by mosquitoes ; but we spent one comfortable night with the Governor, Sir George Denton, and enjoyed " two dinner parties, a breakfast party, a tea party, and a long drive." But, I wrote to my wife, " As I expected, the people have done absolutely nothing against the mosquitoes at Bathurst." The Governor, however, agreed to receive someone from our School to commence the work with the help of my Tropical Sanitation Fund ; and we ultimately sent Everett Dutton—who, however, being a pathologist, did not understand sanitation and achieved little in the line required.

We reached Freetown on 2 July ; and I remember that the first thing we beheld on approaching the wharf in a boat was a cow running up and down with a huge negro holding on to her tail, among the plaudits of the light-hearted populace ! The Governor, now Sir Charles King-Harman, received us most kindly at Government House, until we could hire a house for ourselves in the town, which we did after a fortnight. To my surprise there were not nearly so many mosquitoes in this residence as there were in 1899 ; this was owing to the fact that Captain Hodgins, A.D.C. to the Governor, had cleared the

house and grounds of the larvæ of the pot-breeding mosquitoes before our visit. But there was little other improvement; and on 3 July I wrote to my wife: "We are already starting work. The local authorities have done almost nothing, so far as I have been able to see as yet. We find the breeding pools just as we (Austen, Annett, and I) left them (in 1899)." Holes, puddles, and ditches which could have been drained or filled up at the cost of a few shillings each were still swarming with larvæ exactly as before. Apparently all the available money was being wisely expended to obtain the services of a white health-officer to direct the laborious sanitary work which was not being done.

The Governor himself was evidently not convinced of the mosquito-theory, but discussed it openly with us and suggested that I should give a public lecture upon the subject. This I did on 18 July, in a large hall crammed with people of all colours. The Governor presided. The audience was appreciative, but hilarious; and for some reason a photographic lantern-slide of a mosquito's stomach excited Homeric laughter. A resolution supporting us was passed *nemine contradicente*; whereupon the Governor said he would add more men to our labour force.

Meanwhile we had started work with vigour—the rains having decided to do the same! We were at it all day; and the following extracts from my *First Progress Report of the Campaign against Mosquitoes in Sierra Leone* [64] give the facts briefly:

"Dr. Logan Taylor commenced work without delay. In my first suggestion for controlling malaria I had recommended measures against mosquitoes of the genus *Anopheles* only; but mosquitoes of the genus *Stegomyia* have now been conclusively proved to carry yellow fever; and mosquitoes of the genus *Culex* have long been known to carry *Filaria nocturna* (elephantiasis). Malaria and elephantiasis prevail all down the coast; and many medical men of repute consider that yellow fever also has existed there from time to time. In addition, it is beginning to be thought by some that mosquitoes may carry other diseases, especially various tropical fevers distinct from malaria and typhoid; and, altogether apart from their pathological agency, most kinds of mosquitoes undoubtedly cause an immense amount of annoyance in the tropics, and, next to the heat, constitute perhaps the principal

drawback of life in warm climates. We determined, therefore, to push our campaign against all kinds of mosquitoes indiscriminately.

"Dr. Taylor immediately engaged the services of over twenty men, under intelligent headmen. To these His Excellency the Governor added twelve men, and gave the necessary carts and implements. This force was divided into two gangs; a small gang of six men (called the *Culex* gang), to collect from private houses all the broken bottles and buckets, empty tins, old calabashes, and similar unconsidered vessels in which mosquitoes of the genera *Stegomyia* and *Culex* breed; and a larger gang (called the *Anopheles* gang), to drain the pools and puddles in the streets and the backyards of the houses, in which *Anopheles* breed.

"The *Culex* gang, under a native headman, did very rapid work. They piled the rubbish into carts, which then discharged it into an assigned rubbish shoot. At the same time they showed the larvæ to occupants of houses and instructed them in the manner of destroying them by emptying the vessels which contain them, or by dropping a little oil on the surface of water in which they live. It was found that on the average this gang cleared about fifty houses, and removed about ten cartloads of empty tins and broken bottles daily. The effect of this work on the prevalence of *Culex* and *Stegomyia* can be imagined when it is remembered that about one-third of the tins and bottles contained the larvæ at this season (the rains). Every house had previously been breeding mosquitoes in its own backyard or garden. The occupants welcomed the gang wherever it went, and some stated that they had not been able to get rid of their rubbish for years.

"The *Anopheles* gang had a more difficult task. The breeding-pools of these insects in Freetown, both in the rains and the dry weather, have been minutely described by two previous scientific expeditions. At this season the water-courses contained impetuous torrents too rapid for larvæ to live in; but the streets, yards, and gardens possessed numerous pools of rainwater, well suited for them. These were attacked by many methods. Some were filled with earth, rubble, and turf. Others were evacuated by cutting through the rock which contained them, or by making channels in the

soft earth. Owing to the large rainfall (estimated at about 160 inches annually), to the peculiar nature of the ground, and to the very defective surface drains, these puddles were exceptionally numerous in Freetown; and, in order to drain many of them as soon as possible it was deemed advisable to adopt the simplest and least expensive methods at first, and to reserve more permanent works for the future. At the same time several men were especially employed in brushing out with brooms, or treating with crude petroleum or creosote, those puddles which the workmen had not yet had time to touch. Progress was fairly rapid in spite of the deluge of rain; and many of the worst streets were fairly well drained in a few weeks."

On 20 July 1901, leaving Logan Taylor hard at work, I started in the ss. *Jebba* for Lagos. According to my letters home, we had many passengers who kept their spirits up by pouring spirits down, and one of them acquired a black eye in the process; but one of our students in Liverpool, Mrs. Stewart Deacon, bound for Accra, was on board—she did useful work afterwards on the Gold Coast. We passed close to Cape Coast and touched at Accra—a long dull flat coast, ending in low white cliffs beset by a roaring surf. I intended to take Accra on my way home, and therefore proceeded to Lagos, which we reached on 26 July. We had to anchor in the open, rolled about by heavy surges, but I was saved from going ashore in a surf-boat by the arrival of the Governor's steamer with his A.D.C. and a letter saying " your room is ready." In a couple of hours we had traversed the bar and the lagoon and were met by Sir William MacGregor on the steps of his palatial Government House at Lagos—different indeed from poor Freetown. He wore a white pith-helmet, his ribbons, and a kilt of the MacGregor tartan! Dr. Strachan and several of his officers stood behind him. It was a state-reception!

Of all the men I have met I honour him perhaps the most. His father was a farm-labourer at Towie, Aberdeenshire, and he was born in 1846. Assisted by friends and his own assiduity, he studied medicine at the Andersonian College in Glasgow, graduated at Aberdeen University, took his M.D. in 1874, became Assistant Medical Officer in the Seychelles in 1873, and in Mauritius in 1874, and Chief Medical Officer of Fiji a little later. Very soon, however, he was selected for administrative work, and represented Fiji at the first session of the

PLATE VII.

THE RIGHT HON. SIR WILLIAM MACGREGOR,
P.C., G.C.M.G., C.B., M.D., LL.D.

Federal Council of Australasia in 1885. He was given the Albert Medal and a Medal of the Royal Humane Society of Australasia for saving life during the wreck of a ship. In 1888 he was made the first Administrator of British New Guinea, where he pacified turbulent tribes, explored large new areas, and won the admiration of all. He became Governor of Lagos in 1899.[1]

I met him first at Liverpool in 1899 (page 375), and again when he delivered an address to the London School on 3 October 1900—full of thought and knowledge, and with appreciation of public duties regarding malaria-reduction at Lagos. I now came to him at his invitation, which reached me at Freetown in July.

Wise, grave, but humorous, bearded, thick-set, with wrinkled forehead and a high and somewhat conical bald head, his low voice and kindly manner filled all with trust in him. He drank no wine and did not smoke, but was no fanatic in these respects, and kept a hospitable table. Every night he read from his Greek Testament, and was also skilled in French and Italian, and knew something of many barbarous tongues. He was a mathematician, a practised surveyor, a lapidary, and a master of many arts, but always proud of his medical upbringing and of his nationality. Simply dignified, he did not allow his dignity to obscure his personality, and he had no trace of that meanest and most mischievous vice, jealousy. He was not a politician, but a genuine administrator careful of all the interests of the people entrusted to him—still more, a scientific administrator who added knowledge to his solicitude. He went minutely into every question submitted to him, and it would have been impossible for him to deal with our deputation as Chamberlain had done—in fact he would have made a much better Secretary of State for the Colonies. The only medical man who has been a British governor of recent years (and there ought to be many more medical governors), he recognised the superlative value of sanitation in the development of a colony, and was, I think, the only high British official who ever grasped the real importance of the general anti-malaria scheme which I proposed in 1899.

Well, no sooner had I arrived and been given some tea than he showed me how the whole of Government House was protected by screens of copper wire-gauze to the windows; and called up a little negro boy in a smart livery, whose sole duty it was

[1] See a biography of him by Dr. R. W. Reid in the *Aberdeen University Review*, November 1919.

to catch and kill any mosquitoes which might enter in spite of the screens. Then, still dressed in topi and kilt and attended by interpreters and others, he led me afoot into a neighbouring market, where many large and loquacious ladies, amply clad in yellow gowns, were seated at stalls selling fruit, yams, and dried rats. Bowing and taking off his topi to several of these, he enquired regarding their health (which was quite evidently good), the health of their children and husbands, and the presence of fever in their homes; and introduced me to them. After dinner, we examined with the Hon. Dr. Henry Strachan, his Chief Medical Officer, a number of regulations regarding malaria which he had recently issued—I still possess copies. He ordered all his officers to take quinine regularly and, to set an example, nearly poisoned himself with thirty grains on two days every week. I excused myself from doing likewise, and pleaded the possession of a palm-leaf fan with which I always keep off hungry visitors in the tropics!

Lagos was a much richer city than Freetown and possessed some fine houses, many of which were protected with wire-gauze and lit by electricity; but as they were often neighboured by native slums, while the whole town was on the flat and was surrounded by lagoons and marshes and the rainfall was very heavy, the proposition before us was not an easy one. Sir William MacGregor was draining or filling up some of the marshes and making embankments and outlets at considerable capital cost; but he and Strachan had not paid sufficient attention to numerous shallow rain-pools in the sandy streets close to the houses. I found swarms of *A. costalis* in these. Now according to my law (page 130) that *ceteris paribus* malaria should vary, like light and gravity, inversely as the square of the distance from its origin, a square yard of puddle one yard distant should be as dangerous as a square mile of marsh one mile distant; while the cost of draining the latter might possibly be, at least according to extent of surface, 1760^2 or 3,097,600 times larger. *I* should therefore have begun with the puddles and should have left the marsh for subsequent action if found really necessary. Sir William understood this better after he had visited Ismailia with me next year (page 470).

Another scheme of his had been to found the Lagos Ladies' League for the destruction of mosquitoes in private houses, both of whites and of natives, with Mrs. Sapara Williams as President; and he gave them a grand luncheon on 2 August while I was there. I saw no loss of appetite among the ladies due to the malaria—either for viands or champagne; and the

proceedings were extremely cheerful, our speeches being uproariously cheered by the more sunburnt sisters. But whether the mosquitoes suffered much in consequence I doubt—though the gardens or backyards of some of the members were certainly well kept. He was also passing legislation against the breeding of mosquitoes in private premises—as the Americans had done. All such legislation can do no harm; but in practice the cost of the numerous summonses required to enforce it is apt to exceed the cost of doing the work by departmental agency—as experienced health officers well know. Municipalities favour the idea because it appears at first sight to put them to no expense; but we generally observe two things about municipalities—the excellence of their sanitary by-laws and the completeness with which the public ignores them. Which is the cheaper in the end, (*a*) to make one inspection and then to do the work, or (*b*) to make many inspections, worry the householder, issue several summonses, be finally forced to do the work, and then try to recover the cost in a law-court?

He took me for a two-days' trip in his flat-bottomed stern-wheel government paddle-boat up one of the lagoons to Badagry. It had a trick of veering suddenly to the right and trying to run up the bank; and it did so on this occasion. Fortunately it did not "stick" for long; and I was able to enjoy the cool voyage, the luxuriant tropical vegetation, the huge gorgeous butterflies which came on board, and the store of knowledge of such matters possessed by my host. He was somewhat indignant regarding some of the things, such as this yacht, which the home Government had sent him. They had also supplied him with a locomotive for draining the swamp, which, when it arrived, was found not to fit the rails which had already been laid down at considerable expense. Dr. Strachan also took me a long day's railway journey (120 miles), through forests and past hills, to the great native city of Ibadan. We spent the whole of next day in traversing this immense collection of rambling mud-houses in our "hammocks," carried by negro bearers. The city seemed to be occupied only by legions of women and children, and when I asked where the men were, I was told they were asleep in their houses! We visited another "government failure," a luxurious and expensive iron house which was so unwisely constructed that it became unbearably hot in the daytime, so that the occupants were obliged to go and sit in the shade of trees outside. We slept in a well-made railway house which was said to be very malarious. Not a single mosquito was to be seen; but Strachan swore

that numbers entered in the dead of night. I noticed none because I slept in a good bed-net; but to test Strachan's theory I put one of our servants for the night into an old net with holes in it. Next morning, sure enough, there were five or six gorged *Anopheles* within his net. If we had slept unprotected we might have been bitten by scores of them. This makes a good kind of mosquito-trap for testing the real frequency of *Anopheles* anywhere.

The Hon. C. Tambaki, President of the Lagos Chamber of Commerce, gave me a grand banquet on the evening after the lunch to the L.L.L., and His Excellency gave me another—the results should have been serious in that steaming climate. At the latter I said that "I have been on the point of believing that my countrymen were becoming an unscientific and unpractical people; . . . but I am delighted to find my pessimism is not justifiable as regards Lagos. . . . I came prepared to teach, but have remained to learn" (*Daily Telegraph*, 2 September 1901). In these Crown Colonies the Governor is everything, and as the Governor is so are the officials and the people. I remember chiefly Bishop Tugwell, Mr. C. H. H. Moseley, the Colonial Secretary, and Drs. Strachan, Pickles, Rice, and Best, all capable medical officers. Sir William himself described the work in detail shortly afterwards (see *British Medical Journal*, vol. ii, 1901, page 680, and my *Mosquito Brigades*). I was to meet him again soon on happy occasions; but the low land of wood and water, swept at that season by tropical rain from leaden skies, has only sunny memories for me.

On 5 August I left Lagos, with many regrets, in a pleasant German steamer in the midst of appalling rain, for Accra; and remember being welcomed on board by the Captain, the Purser, the Doctor, and the Chief Steward in a line—so different from the chilly reception one often meets with when coming on board ship—and by a number of officers from German West Africa, who talked philosophy to me in English and drank beer to me in German; the military doctors knew all about the work of Koch and myself. We arrived in about two days at Accra, where I stayed for three days with the Governor, Major Matthew Nathan, R.E., in order to arrange for sending someone to start anti-mosquito work there like that of Logan Taylor in Freetown. This part of the coast is much more dry than Freetown or Lagos, but is still very malarious owing to the breeding of *A. costalis* in the "swish-pits"—holes in the ground from which clay is taken to build native houses. The Europeans were well housed; and there is a remarkable old massive

structure of several stories, called Christianborg, built by the Danes close to the sea and bathed by the spray and mist rising from the thundering surf—a grand but melancholy pile in which many Europeans have died. The Governor, who was very kind (he ruled over Sierra Leone in 1899), insisted on my playing polo; but as I had not ridden for six years I was no good to my side. He wisely encouraged all kinds of sport, and it was possible to keep ponies at Accra. Of course he was already converted to "mosquitoism," and was keen on starting work here.

From Accra I went in the steamship *Oron* to Freetown, which we reached on 16 August. I stayed with the Governor, and after I had examined the work done by our men, he asked me to consider with him his project for making houses for his officials and other Europeans on the hills to the west of the town. At my first visit I had wondered for what conceivable reason this had not been done years ago; but opinions were divided on the point. Liverpool merchants generally wished their agents to live near their offices in the town, for business reasons; and others thought it wicked for white men to segregate themselves from their brothers, being quite regardless of the fact that the former died so frequently in consequence of the proximity. I was in favour of the new settlement being higher up the hills; but a railway was required, and Sir Charles King-Harman finally chose the spot referred to—about 500 feet above sea-level.

McKendrick and I left Sierra Leone on 21 August in the ss. *Sobo* and reached Plymouth on 2 September. Long before this I had begun to suffer again from the tropical malaise due to the heat, which had persecuted me in India and which debarred me from thinking of permanent work again in hot climates. I remember nothing of the voyage home except that there was an obnoxious little journalist on board (? or on the *Oron*) who insisted on disproving the mosquito theory to me and, when I closed the discussion from very weariness, threatened to "expose" me in the press—which he actually did in *The Standard* a little later.

I wished to procure an independent opinion upon the results of our work, and had therefore asked Dr. C. W. Daniels, then Superintendent of the London School, to visit Freetown. He arrived shortly after I left for home, remained some weeks, studied our operations with great care (little more than two months after they had been commenced), and reported to me after his return to England, as follows:

1st October, 1901.

DEAR ROSS,

I have carefully examined the various works which have been undertaken with a view to the serious diminution in the number of mosquitoes in Freetown, Sierra Leone. . . .

In my opinion, already your efforts have been crowned with a large degree of success, as there has been a noteworthy diminution in the number of the first two genera [*Anopheles* and *Stegomyia*] found in the houses. The number of breeding grounds has been enormously diminished.

The operations, having been only recently begun, are, of course, as yet far from complete. A considerable part of the town, perhaps half, has not been touched. Even in the parts longest under treatment, in the yards adjoining the streets, there are still numerous breeding grounds; and in the streets themselves occasional places have either been overlooked or the works undertaken have not been effective as yet. . . .

Though I consider that you have already proved the practicability of exterminating *Anopheles* in Sierra Leone during the wet season, the work is at present incomplete, even in the streets in which most work has been done; and, I estimate, at the present rate of work, will still be incomplete at the end of the wet season, when the work will be entirely changed. During the dry season, in addition to dealing with the new conditions which will then arise, the work already done should be placed on a permanent footing.

In the next wet season double the men, say one hundred, should be employed, and two Europeans for supervision. One European, even so able and energetic a man as Dr. Logan Taylor, barely suffices for thorough supervision of the present work.

I am aware that this will cost, apart from the expense of supervision, over £100 a month instead of the £50 or £60 which, including the cost of labour provided locally, is now spent; but it will be better for one place to be done well, and that a difficult one to deal with, than that partial measures be attempted in many places.

The experiment is being so closely watched and criticized that failure, or only doubtful success, would be a disaster. . . .

In conclusion, I wish to express my thanks to you personally, and to the Liverpool School of Tropical Medicine, for the opportunity afforded me of seeing the first real British practical application of the principles you have elucidated.

I am,

Yours very sincerely,

C. W. DANIELS, M.B.

London School of Tropical Medicine.

The full letter is given in my *First Progress Report* (page 442), but most of it is occupied with details which we had already considered. The penultimate paragraph quoted above showed that Daniels did not quite grasp our intentions. We were not making an experiment, but giving an object lesson, and therefore (fortunately) I added the following footnote to the end of his letter:

"In order to guard against misapprehensions, it is advisable to state here that we are not now undertaking to prove over again that mosquitoes carry malaria. This fact was fully established long ago. Our present intention is simply to give an object lesson in the manner of ridding tropical towns of mosquitoes by drainage and cleaning up. We are prepared to spend a large sum of money for this purpose; but we are not prepared to continue the work for ever. The work—especially the drainage and collection of rubbish—properly belongs to the local authorities. If they choose to continue our efforts, then we can confidently promise that the mosquito-borne disease in Freetown will be, ultimately, very materially reduced. If, however, they discontinue them—if they allow the town to sink back into the condition it was in when we arrived—then I can only say that the mosquito-borne disease will remain. It is for them to choose. I may add, however, that I have no doubt that the former course will be the one adopted.

"R. Ross."

Manson, too, did not understand the matter in the least, for he wrote to me on 14 October 1901: "Daniels believes in your mosquito campaign but is *very very very* strong on not risking failure and on *concentration* in our plan. This case (?)

good and the British Public will do the rest; this a failure and good-bye for years to mosquito sanitation. *Verb. sap.*" Both of them (and many others) seemed to think that I was trying to prove the possibility of banishing mosquitoes from Freetown *for ever and ever at a single blow.*

It will be seen later what the "local authorities" actually did do (page 485). Accounts of our work will also be found in *The British Medical Journal* 7 and 14 September 1901.

As this and the simultaneous campaign of Gorgas at Havana were the first of the kind attempted, it may be interesting to give another extract from the same *Report* [64]. An account of the results, after my third visit, will be found on page 461.

"It may be advisable to correct some popular errors regarding the operation of clearing mosquitoes. No one has ever supposed it possible to exterminate mosquitoes from whole continents, or even from large rural areas—the operation must be confined principally to towns and their suburbs. No one imagines that it will be possible to exterminate *every* mosquito even from towns—we aim only at reducing their numbers as much as possible. No one supposes that it will be invariably possible to drain or otherwise treat every breeding place of mosquitoes in a town; but even where every place cannot be dealt with, it will always be possible to deal with a very large number; and it often happens that the smallest and most easily drained or emptied puddles or pots breed the greatest number of mosquitoes. Mosquitoes may possibly be carried into towns from a large distance by winds, though I doubt whether there is much or any reliable evidence in favour of this view; but, as a general rule, the vast majority of mosquitoes existing in a town are bred in the streets, yards, gardens and houses of the town; and if we get rid of these breeding places, we may calculate on at least greatly reducing the insects in the town. These are the simple principles upon which our efforts are based."

I conclude this chapter with Gorgas's first letter to me—on the same subject.

HEADQUARTERS DEPARTMENT OF CUBA, OFFICE OF CHIEF SANITARY OFFICER OF HAVANA.

HAVANA, CUBA,
Nov. 19th, 1901.

DR. RONALD ROSS,
LIVERPOOL SCHOOL OF TROPICAL MEDICINE,
LIVERPOOL, ENGLAND.

DEAR DOCTOR,

Your pamphlet " Campaign against Mosquitoes in Sierra Leone " [64] received. I am much obliged for the paper and for your kind felicitations.

It is, of course, a matter of great interest to us at present. The work here has been much more successful than I had hoped when we started. There seemed to me very little prospect for accomplishing much when we commenced, in February of this year; but, as you will see from our reports, our results have been most positive. For the first time since the English occupation, 1762, we have an October free from yellow fever, and malaria decreased more than one-half.

Mr. Le Prince, directly in charge of mosquito work, estimates that mosquitoes have been decreased 90% by the work, as compared with this time last year. Of course, this is a difficult statement to substantiate; it is a matter so much of individual opinion. But I have convinced myself that they have been very greatly decreased. My own quarters on the bay front, where they have always been very bad, have had none, practically, for the last six months; and I know many other localities where similar positive statements can be made.

But this is certain: that last October we had 74 deaths from yellow fever, this year no deaths and no cases; and from malaria fever, last year 25 deaths, this year 19. This, I am convinced, is entirely due to the mosquito work.

This disappearance of yellow fever, however, I think is almost altogether due to the killing of infected mosquitoes at the infected point. We do this by burning pyrethrum powder in the infected house and all the neighbouring houses. It has been extremely gratifying to see how promptly the focus of infection is stamped out in this way; and it has been likewise surprising to me. I knew that some mosquitoes must escape from the most careful mosquito-hunt; but we have apparently entirely controlled the disease this year by this method, when the conditions were exceedingly favourable for its spread. It

must be that there are only a few infected mosquitoes in each individual case, and that they remain pretty close to the point of infection. And this probability is rendered greater, if we consider that it takes a mosquito 15 or 20 days after biting, before he himself is able to transmit the disease. If 50 mosquitoes bite a yellow fever patient, it seems to me quite probable, from natural causes, that only 4 or 5 would survive the 18 or 19 days required to render them dangerous.

With kindest regards and many thanks, I remain

Yours very sincerely,

M. C. Gorgas.

Major & Surgeon, U.S.A.,
Chief Sanitary Officer.

After all there is something to be said for Utilitarianism. Our efficient grandfathers commended it; but we deride it and prefer the opposite philosophy which may be called Fakirism—that lofty spirit which loves to sit by the wayside lost in the beatific contemplation of infinity and its own virtues and quite indifferent to such petty evils as heat, cold, hunger, and vermin. I fancy that, but for the yellow fever discovery and the importunity of some of us, no mosquito-reduction against malaria would have been undertaken even yet in our colonies—just as none (so far as I know) has yet been undertaken against the hideous elephantiasis, even after nearly fifty years of scientific demonstrations (page 127).

CHAPTER XXIV

WEST AFRICA, ISMAILIA, AND STOCKHOLM, 1902

THUS the work commenced in May 1895 was now brought to its logical and practical issue in August 1901.

I had been appointed President of the Tropical Disease Section at the annual meeting of the British Medical Association to be held at Cheltenham from 30 July to 2 August 1901, but could not attend owing to absence in Africa. There was a discussion on the prevention of malaria, at which papers by Sir William MacGregor describing his measures at Lagos, and another by J. M. Young on J. C. Thomson's work at Hong-Kong were read. I sent a Note on the "Habits of Europeans in India and Africa in Relation to Malaria," in which I pointed out that the "habitual use of punkahs, mosquito-nets, well-built houses, and comparatively good food" by Europeans in Indian cities puts them at an advantage compared with their fellows in Indian plantations and the African coast towns. They are also generally segregated from natives and possess gymkhanas, clubs, dairies, vegetable farms, "mutton clubs," ice, and soda water in the larger Indian stations, while many of these were frequently wanting in African settlements; and I argued that "the slow progress of the African colonies is chiefly due to the stupid indifference to these details" and not so much, probably, to any greater "disease-potential" there. I might have omitted the word *stupid*, but the criticism was quite sound and valuable. Truth is however an unpopular goddess, and my paper was only "taken as read"! C. F. Fearnside described some mosquito-inoculation experiments, including one on himself (*British Medical Journal*, 14 September 1901).

One reason why I had hurried back from Africa was that I had promised to read a paper on malaria on 18 September 1901, at the meeting of the British Association at Glasgow. The paper ended with an analysis of the various methods of malaria-prevention and an account of our work at Sierra Leone. Lord Lister moved the vote of thanks to me, and

I cannot refrain from quoting his very kind words. "Major Ross," he said, "had tried to prevent malaria by getting rid of the water pools in which the mosquito deposited its eggs and in which the larvæ developed. This task had seemed hopeless to many, but the results already obtained were an absolute proof of the efficacy and practicability of the means adopted. He wished to bear his testimony of admiration to the qualities which had enabled Major Ross to bring about this great discovery, because the discovery of the development of the parasite in the mosquito was due solely and simply to Major Ross, who had shown admirable scientific acumen, zeal, and perseverance. At the same time he had—very differently indeed from some Italian investigators—shown absolute candour, perfect openness of mind, and a readiness to recognise the work of others" (*Times*, Thursday, 19 September 1901). I hope the reader will excuse my vanity, but I look upon these words as the red-ribbon of my life!

We had some delightful excursions round Glasgow, especially one through Loch Achray, Loch Katrine, and Loch Lomond—the very centre of romance. It was then that I met my friend, Professor Charles Jasper Joly, Royal Astronomer of Ireland and editor of *Hamilton's Quaternions* (page 415)—who was very kind regarding my *Algebra of Space*. We were a merry party, and when our train stopped at Bannockburn I enquired of the ticket-collector, for a joke, if he could tell us whether this was the place where the English had defeated the Scots so grievously in a great battle long ago. Our carriage was full, quite evidently, of mere Southerners; and he looked round us gravely and merely replied: "Well, sir, I'll no be hurrting anyone's feelings heerr."

Of course I took the opportunity of calling on Mr. Coats at Ferguslie House, Paisley. He wanted me to go yachting with him for a few days, but I could not spare the time. He was a genial simple man, somewhat lame, and was very pleased with our work at Freetown.

I received several letters in 1901 on the priority question besides those from Koch and Laveran quoted in Chapter XXI. Dr. Mannaberg of Vienna, one of the very best writers on malaria, wrote to me on 6 June: "It seems that the Italians are attempting once more what they already did twelve years ago with Laveran." On the same date Dr. E. Almquist of Stockholm, who had asked me for papers on the subject, wrote: "In the question of priority there cannot be any doubt. The matter of fact is so easy to demonstrate, and

Koch and the greatest authorities are of your mening." On 17 June Professor Dr. Galli-Valerio of Lausanne, a capable worker, wrote: "For me, it was not necessary to read it (*Letters from Rome*) to have the conviction that the merit of the great discovery is to Major Ross. And I am sure that many other Italians think the same." All these letters were in English.

Immediately after my return to England in September I wrote my *First Progress Report* [64], quoted in the previous chapter; and then I compiled my little book called *Mosquito Brigades and how to Organise Them* [68]. It was intended to enable any health officer or medical officer in the tropics to do the work which we were doing in Freetown. It discussed and compared the various methods of malaria-reduction and of mosquito reduction in general and described campaigns then being conducted. I received many letters about it, and the whole edition was gradually sold; but I fear that it did not stimulate as many men as I hoped it would.

On 13 October 1901 my son Charles Claye was born at 36 Bentley Road. A little later we moved into 26 Devonshire Road, also near Princes Park, Liverpool.

About the same time, according to my promise, the school sent Dr. Everett Dutton to the Gambia, where I hoped he would help the Governor (Sir George Denton) to start anti-mosquito work, and I furnished him with money for that purpose out of my Tropical Sanitation Fund. He arrived at Bathurst on 6 October, found *Anopheles* larvæ in many places, including swamps; and helped the local authorities to organise the work. But the Governor decided to pay for it himself, and our money was saved. Dutton was an able and enthusiastic investigator, and in December 1901 discovered, with the Colonial Surgeon, Mr. R. M. Forde, the first trypanosome in the blood of man—*Trypanosoma gambiense*, which was later proved to be the cause of the dreadful "sleeping sickness." I possess Dutton's letter announcing this important discovery to me. He was well satisfied as to the way in which the local authorities were taking up the malaria work, which was peculiarly difficult at Bathurst; and having completed his task returned home about the end of January 1902.

I had promised Major Matthew Nathan, Governor of the Gold Coast, to send someone to him to do the work that Logan Taylor was doing at Freetown. Mr. F. Swanzy very generously added £500 to my fund for this purpose, but I had great difficulty in finding anyone to take the post. At last I accepted

a man on the recommendation of Boyce, and he arrived at Accra on 17 December, but without stopping at Freetown to learn the work with Taylor, as he had been instructed to do. He then told the Governor that he was not prepared to do such work at all, as it was not " a medical man's work," but that he wished to investigate disease, and required extra payment even for that. He also desired to be made a sanitary commissioner. The Governor, who had long been expecting help from us, was annoyed, and so were Mr. Swanzy and ourselves. All we could do was to withdraw this doctor and to send Logan Taylor to the Gold Coast later, where he worked as well as he had done at Freetown.

On 21 October Mr. Jones gave me a lunch at the Exchange Station Hotel in Liverpool, after which I delivered an address to the African Trade Section of the Chamber of Commerce [65] (printed by the Chamber). In this I reiterated the advice to Europeans in Africa which I had given at the British Medical Association meeting—I suggested not only mosquito reduction, but everything which would add to comfort and therefore health, from soda-water to gymkhanas. " If," I said, " our countrymen in Africa continue to live, as too often they live now, in the midst of marshes and other squalid surroundings, without exercise and recreation, without good food, punkas, and other comforts, then the general mortality among them will remain high "—in spite of sanitation. Many of the business men present told me that they would act on my advice regarding their employees.

At that time several writers were attacking me in the London papers ; some seemed to think that I was to blame for the fact that mosquitoes carry malaria, and others that I was a charlatan who was amassing a large fortune by my pretences. The doctors were jealous, the medical press was cold, and the Colonial Office adamant. Even Sir William MacGregor was not encouraged and had written me on 14 April before I visited Lagos :

" I shall be very glad to send you a few notes on malaria in the field, what we are doing, and what lions roar at us in the path. So far the greatest obstacle is the Colonial Office. It is bad enough to have to deal with malaria alone ; but malaria entrenched behind Sir ——— is impregnable. The S. of S. approved of my dealing with the railway ; but before I reached this the permission was withdrawn. It has now been restored in a modified form."

Boyce, Milne, and I felt (then and afterwards) that secret enemies were working against us in London; and I finally wrote to the Colonial Office asking to be allowed to keep in touch with it regarding sanitary affairs; it had never approached me. I received the following reply:

COLONIAL OFFICE,
Dec. 22, 1901.

MY DEAR MAJOR ROSS,

I am in receipt of your letter of the 16th instant, and shall be very glad if you will write to me at any time when you have any information which might be useful to us in the Colonial Office but would hardly be of sufficient importance to form the subject of an official letter. And I hope that, when you happen to be in London, you will sometimes look in and see me, if you have a few minutes to spare. I am most anxious to keep in touch with what is being done in connexion with tropical sanitation, but I have hardly time to read all that ought to be read and a short conversation often teaches one more than much reading.

Believe me,

Yours very truly,

. . . . .

When I went to see this gentleman, however, he informed me straight away that he believed in the mosquito-theory but was sure that we should find it quite impossible to banish mosquitoes from Africa. I agreed; but as it was clearly equally impossible to banish this kind of intellect from the Colonial Office I decided that I could not afford to travel often from Liverpool to London (at my own expense) for such interviews.

About the middle of November Jones was knighted (K.C.M.G.). He was one of the godfathers of my son Charles, and was at the christening, which was performed by the Bishop of Liverpool (our much honoured Dr. F. J. Chavasse).

The year 1902 was full of events for me. Although everyone in Liverpool had been extremely kind to us and the citizens had subscribed handsomely to the School, my position still remained that of Walter Myers' Lecturer in Tropical Medicine at University College, with a salary of £300 a year; and there was no evident desire to increase my pay and much less to endow a professorship and a pension for me, as Boyce had

promised when I left the Indian Medical Service for Liverpool in 1899. The School was enjoying large subscriptions, chiefly owing to the fame which my work and ideas had given to it. Thus *The British Medical Journal* of 6 May 1899 announced a subscription list which had then already amounted to £2,500 a year. I could not be a member of the committee, and therefore did not know exactly how the money was being spent, but thought that much of it was being used rather fruitlessly and that I was being exploited—and so, I may add, did many others, including Lord Lister and Sir William MacGregor. The position was not improved when attempts were made to force me to give my book on *Malarial Fever* to the School (page 391). I had many expenses, especially during my recent visit to Africa, which were not met by my pittance. I thought I had done enough for malaria and West Africa for the money, and proposed now to take consulting practice in London. In December 1901 I therefore threatened to resign unless my position could be improved.

While this was being considered, my friend, Dr. Allan Macfadyen, secretary of the Jenner Institute of Preventive Medicine in London, suggested that I was wasting my time over teaching and that it would now be better for me to join his institution for research in parasitology. I assented provisionally, but said that I must first return to Africa for a short period, in order to inspect Logan Taylor's work there. Lord Lister, the chairman, sent for me on 12 February 1902 to consider details with him, and on the 19th gave me the conditions of the appointment in writing. I was to be the head of a new department of animal parasitology, with a laboratory under my direction, a salary of £500 a year, and an assistant at £200 a year—much better than the Liverpool post. His letter reached me just before my departure for Freetown on 22 February. I had no time to consider the details, but accepted the offer, and, just as our ship was casting loose, told Boyce, who came to see us off, that I had done so. He and Milne were troubled, but evidently scarcely believed me.

On 7 February, with the consent of the donors, I had handed over the whole residue of my Tropical Sanitation Fund, with accounts and vouchers complete, to the School, for future administration under the conditions for which the money was originally given. I could not afford to pay my own expenses again for this my third visit to Africa, and Mr. Coats had therefore given me permission to draw up to £50 for this purpose. We had heard such fine accounts of the work at

Freetown, both from the Governor and from Logan Taylor, that I decided to take my wife with me this time, just to show that I, at least, did not consider Freetown to be any longer "the white man's grave."

But we feared (or hoped) that we were going to die on the way out! We were put into a nice cabin (? s.s. *Fantee*) in which the stewardess, who had a bad cold, had slept in port. A day or two later my wife and then myself went down with the most appalling malady of the same class, which prostrated us until we neared Freetown. On landing there on 7 March the heat and dust produced a relapse worse than the original attack. Sir Charles and Lady King-Harman were kindness itself; but the upper story of Government House, in which we slept, was dreadfully hot; and we were more or less ill during the whole of the nine days we spent in Freetown, and scarcely revived until we approached Liverpool again at the end of March. These are the facts; but our friends the sceptics in Sierra Leone immediately put it about that we had been attacked with malaria there, and this lie actually appeared in the papers. It was afterwards traced to its source —a doctor in Freetown who knew the truth of the case but had always opposed the anti-mosquito campaign. I was just able to inspect Logan Taylor's admirable work and to attend a garden party and a dinner which the Governor gave for us, but was obliged to abandon the visit to Bathurst which I had hoped to make on the return journey. An unlucky trip.

On 15 April 1902 I described the results obtained in a letter to Jones, which was published *in extenso* in *The Liverpool Courier* of next day under the sub-heading, "Garden Parties at the 'White Man's Grave.'" I believe it was never published elsewhere, and therefore give some extracts from it, because they sum up the subject tersely. Speaking of Taylor's work I said:

"Employing about seventy men (of whom twelve have been lent by the Governor, the rest being paid by the school), he has drained nearly the whole of the most pestilential parts of the town. These parts were the flattest areas of the valley in which Freetown lies, and were full of hollows, pits and ill-made drains which in the rainy season contained numerous pools of stagnant water, breeding swarms of the malaria-bearing mosquitoes, *Anopheles*. So shocking was the condition of affairs that many of the streets were practically

marshes in the rains, the houses being situated in the midst of seething puddles full of mosquito-larvæ, frogs and tadpoles. The work of levelling and draining these places had been rendered the more arduous by the rocky nature of the ground and the faulty nature of the old municipal drains. In fact it was very largely these drains which were responsible for the bad sanitary reputation of the 'white man's grave,' many of them being huge square trenches without any adequate fall and with innumerable 'pockets' holding filthy water for months.

"How anyone expected that Freetown would be otherwise than unhealthy when in such a condition it is difficult to imagine. Dr. Taylor has now reclaimed most of the pools by cutting even channels and filling up pockets with rubble and cement, but it should be understood that it will take a considerable time to render the work permanent. I am therefore happy to say that the Government has generously undertaken to deal with two of the worst streets in a permanent manner out of their own funds, and to extend the work gradually year by year. In the meantime, such pools as have not yet been drained, or which cannot easily be dealt with by drainage (e.g. pools in the course of the mountain streams, cess pits, and old wells), are kept free of mosquito larvæ by being periodically treated with crude petroleum by a special gang of men employed by Dr. Taylor for the purpose.

"In addition to this kind of work, the school employs another special gang of men to remove old tins, bottles, and similar rubbish from the houses. Dr. Taylor informs me that these men have now removed 2,257 cartloads of such rubbish, and have visited 16,295 houses—the latter figure implying, of course, that all the houses in Freetown have been revisited periodically. All this has been done by about half-a-dozen men in eight months. I consider this to be a most useful piece of work. Most of the tenants of the houses say that their rubbish has not been removed for years; and it can easily be imagined what swarms of mosquitoes formerly bred in two thousand cartloads of old tins, etc., at least a third of which probably contained the larvæ during the rains. All this rubbish has now been either dumped in the sea or used for filling up insanitary pits and hollows.

"It will now be asked, what effect have these measures

had, after eight months' trial, on the number of mosquitoes in Freetown? In my opinion we have absolutely demonstrated the possibility of getting rid of mosquitoes at a small cost in Freetown, and therefore probably in any town. . . . By getting rid of mosquitoes I do not mean that not a single mosquito is to remain. Such is, of course, an absurdity, because, even after the most careful search, a few breeding places will always remain undetected, especially in the houses of people who persist in keeping stagnant water in pots and tubs, and who are too lazy or stupid to look after their own comfort. I mean that the number of insects can be reduced so largely in towns that they will cease to be a cause of disease and discomfort to the bulk of the population.

"We will not attempt to give any numerical estimate of the decrease of mosquitoes in Freetown because there is probably no sound way of computing the actual number of mosquitoes anywhere; but the general consensus of opinion is that they have been very greatly reduced there. A number of people, both Europeans and natives, informed me that they had seen no mosquitoes for months. In our rooms at Government-house, where I had been frequently bitten on previous visits, we neither saw nor heard a single mosquito during nine days, though the house is surrounded with trees and though our windows were kept open all night. I never remember to have had a similar experience during sixteen years' life in the tropics. Natives residing in the recently-drained areas told me that there were no longer mosquitoes in their houses. I requested Mr. Shaw, Dr. Taylor's assistant, to procure for me as many *Anopheles* as possible from the houses of the natives for study, but after several days' search he could bring me only one! though he and his men had searched a number of houses built close to the streams, in which houses Drs. Christophers and Stephens had found swarms of *Anopheles* in 1900. Of course, as already mentioned, mosquitoes must still be present here and there where their breeding places have not been detected (and in fact a few such are sometimes seen in a corner of the verandah of Government House itself; but there is undoubtedly a vast reduction of them in the town as a whole, and this is the main thing. In the rains, however, we must expect a temporary increase. . . .

" . . . As a matter of fact these insects are strictly local in their ordinary habits, and it is obvious that where we reduce their breeding places to any great extent, we must also reduce their numbers to a similar degree. I fancy that the breeding places in Freetown have been reduced by at least ninety per cent., and it would be surprising if the insects remained as numerous as before.

" As an instance of the imaginary difficulties cited against us, I may mention the case of the mountain streams, in which numbers of *Anopheles* larvæ were found in 1900. It is, of course, impossible to drain these streams, and hence it was immediately assumed that this fact would be an insuperable bar to operations against mosquitoes. Dr. Taylor, however, tells me that six men suffice to keep these streams free of larvæ within the town by periodically brushing out or oiling the pools. Out of the town area the streams are simply left alone, such mosquitoes as breed in them being too few to trouble the town even if they could easily reach it through the forest and bush. Contrary to the popular idea, mosquito larvæ are not usually found in such swamps as exist near Freetown.

" The cost of all these operations has been very small. Labour costs only about one pound a head per month. To this must be added the salary of Dr. Taylor as superintendent; but under normal circumstances the health officer and engineer should superintend the work. After the drainage system is complete, the number of men employed in the campaign against mosquitoes can of course be reduced. One of the principal causes of expense should be the quantity of cement required for the works; but owing to your gifts of this material we have spent little on it. The expenditure of oil, in spite of its extensive use by Dr. Taylor, has proved to be very small; only one and a half barrels out of the tons of petroleum and creasote generously provided by Mr. John Holt having been used as yet. In fact crude petroleum is a most economical culicicide, an ounce or two sufficing for many puddles. Indeed I am sure that by far the most economical way of dealing with malaria in towns, and the best, is by clearing away the breeding places of the larvæ of the insects which carry the disease. . . .

" Now as regards the effect of the anti-mosquito measures

on the health of Freetown :—I have previously said that we must not expect a sudden disappearance of a relapsing disease like malaria similar to the sudden disappearance of yellow fever which followed anti-mosquito measures in Havana. . . . Nevertheless it is generally admitted that the health of Freetown has been recently remarkably good. A number of people assured me that they had not suffered from fever for a long time.

" Dr. Taylor, though he has been constantly engaged in ' turning the soil '—a thing which some people imagine is sure to produce fever—has not had a single attack since his arrival. But what struck me most was a great change—so it appeared to me—in the demeanour of the Europeans. On my first visit two and a half years ago I never saw a more gloomy place, and an officer assured me that they felt ' as if a sword were hanging over their heads.' But now the Europeans looked certainly as well and cheerful as they look in India. At several gatherings I noted only one or two sickly faces, and these belonged to persons forced to live in the hot and unsuitable houses of the lower town. Garden parties and other entertainments were following each other in quick succession, and, most suggestive of all, there were a number of English ladies in the place. Just before my arrival a ball had been given at the Botanical Gardens, at which eighty Europeans, twenty of them ladies, were present, dancing being kept up until 2 a.m. in spite of the so-called ' malarial vapours of the night,' and, needless to say, no one died in consequence. How far this improvement is due to the drainage operations I cannot say. I think that much of it is due to the initiative of the Governor and Lady King-Harman, who are doing their best to pull the place out of its old rut of despondency ; but I also think that the general health of the Europeans is distinctly improved, and that at all events they feel that something is at last being done for their good. . . ."

When this letter reached Sierra Leone it was received with the usual protest by a writer in *The Weekly News* of that colony, who, speaking as an " old resident," attributed the improvement to " general sanitation " and " flatly contradicted " the hypothesis that he was at all more cheerful now than before ! To this the editor added the note : " We take the liberty of

saying that we have been here quite as long as our Correspondent; having been born here; and we are in a position to state that the Sanitary activities of Major Ross and Dr. Logan Taylor have had a marked and important effect upon the health and spirits of the Europeans—at least of all whom we have had the honour to meet." I had given some health statistics in my letter to Jones, but of course no decision could be based upon such figures as had been kept in Freetown by the medical authorities there.

The Jenner Institute proposal proved to be a very trying fiasco. Lord Lister's letter, which had reached me just before I left for Africa on 22 February, gave only a brief outline of the appointment; but the terms were so much better than anything which then seemed likely to mature in Liverpool that I accepted them at once. When I returned, everyone there very kindly begged me to reconsider the matter and declared that an endowed professorship would soon be arranged for me, and so on; but I felt bound by my promise to Lord Lister, and consequently resigned the Liverpool post on 3 April 1902, and went to London on about the 21st. Now, however, I found that the details of the new appointment were not so satisfactory as I had imagined—while Lord Lister had gone away to South Africa! There were no suggestions of rise of salary after service, or of any pension, and the tenure was not *aut vitam aut culpam* but simply terminable at six months' notice; while I was apparently to be placed under the orders, not only of the committee called the Governing Body of the Institute, but under individual members of it—one of whom, a layman, seemed to be very active in his superintendence of the labours of the scientific helots. All this might have been good enough for newly qualified doctors, though scarcely for a man of forty-five, as I then was; but there was an alternative open to me—to spend the small remainder of my capital on commencing consulting practice in tropical medicine in Harley Street. So, as the Jenner Institute refused to modify its conditions, I resigned the post on 1 June. In the meantime, however, I had mentioned my difficulties to my friend, Professor Sherrington of Liverpool, who immediately set to work to get me back there, and with Boyce and Milne, soon collected the guarantees for a professorship. They were so persuasive, I heard afterwards, that even the most generous citizens of Liverpool used to fly at the sight of any one of them! On 1 July I was reappointed Walter Myers Lecturer, but now with a salary of £600 a year; and on 2 December 1902, the Council

of University College invited me "to accept the Sir Alfred Jones Chair of Tropical Medicine"—so called because Jones had promised £5,000 towards the endowment. I remember with gratitude the efforts of all these kind people—not less those of the Jenner Institute and Lord Lister,[1] who did what they could; but, secretly, I always hankered after Harley Street.

These petty details have been mentioned in order to show how we keep science-workers in their proper place in England. I heard afterwards that the kind Liverpool people—who had forgotten for three years their promises to me, though I had brought them some unwonted fame—were rather irritated because I had sought to better myself by going to the Jenner Institute, and that the Jenner Institute was irritated because I returned to Liverpool for the same reason—science-workers should take what they get and be thankful. When I was in London later I was at a dinner where Jones spoke; afterwards I overheard two men talking in the cloak-room. "Who is Great-Man Jones?" asked one. "Don't you know?" replied the other; "he runs Ronald Ross, the mosquito-crank," or words to that effect! I ought really to have gone to Harley Street. The proper work of a doctor is doctoring; his practice is an empire, his consulting-room a laboratory, his reputation a pension, and he is his own master. But it would have been a fight at first, and I was already dead-weary with the triple battle just finished. So it was that I fell before academical blandishments. Young medical reader, take warning!

When Robert Koch discovered the cholera bacillus in 1883 (at the age of forty) the German Government at once made him independent with a large pecuniary reward and a good appointment, so that he was able to devote himself in future to any line of work in which he saw the best promise of success—which no one else could choose for him. Committees and institutions cannot foresee discovery and therefore seldom reach it; in science, as in poetry and war, the individual is everything, the only measure of merit is achievement, the only guarantee for the future is the past. But our politicians are not so wise, and often waste their proved workers on puerile or ancillary labours controlled by indifferent or inexpert committees. Thus, for myself, instead of using me for the large sanitary schemes as I desired (page 389), my countrymen

[1] I have my own opinion (which I refuse to state!) as to why he took the action he did. He wrote me a charming letter when he returned from Africa and I always bless his great memory.

offered me only three minor occupations—to teach students, to dissect parasites, or to prescribe pills. The British are a practical people; they seldom actually kill the goose that lays the golden eggs; they force her to lay goose's eggs!

But this concerned the millions of sufferers more than me personally—I should be saved anxiety and many tedious and dangerous visits to the tropics. The goose herself probably finds it more easy and profitable to lay goose's eggs; and the schools are full of these fine birds all pleasantly so engaged! Therefore, as little value seemed to be set upon my golden egg, I determined to take a rest and join the happy throng cackling on the common.

On 26 June 1902 my name appeared in the list of coronation honours of His Majesty King Edward VII—as Companion of the Bath (Civil Division), my father's Order; but I could not be present till the King's levee of 24 October at Buckingham Palace to receive it. I went in my full-dress uniform of the Indian Medical Service which I had not worn for many years and which had shrunk so unaccountably in the interval that I could scarcely move my head. Our debonair King was seated with his gorgeous officials behind him (I think Lord Roberts was there), and as we Companions filed past him he received us each with a pleasant nod and smile and put the box of insignia into our hand.

In July and August 1902 we had some delightful experiences. In 1901 my colleague, Rubert Boyce (page 371) had married a daughter of Mr. William Johnston, a rich Liverpool shipowner, who now asked Boyce, some other men, and myself to take a ten-days' trip in his large steam yacht, the *Gitana*, along the west coast of Scotland. Mr. Johnston was a kindly man full of amusing stories, but was a strict teetotaller and somewhat puritanical; he kept a lavish table but allowed no alcohol on board, and all his crew (about twenty men I think) were compelled to adopt the same faith—I never saw a more fat and rubicund set of sailors in my life, like a flock of nautical capons. His guests, including myself, were not at all sorry to be "dry" on board, but in some strange way did desire a glass of beer when we touched shore; and it was sad to see how Mr. Johnston was left alone with his principles in his launch at any landing jetty, while we all rushed off to do important business at the post office (we had a glass of beer together *en route*). We left Liverpool on 13 July 1902 I think; spent a day first at Lamlash in Arran with its fine rugged hills; steamed up Loch Long on the 17th, and walked across to

Loch Lomond at Arrochar; then round the Mulls to Oban. Here I received a letter from Professor Sir Thomas R. Fraser of Edinburgh telling me that I should be there on the 26th to receive the Cameron Prize of £80 which the Edinburgh University intended to present to me. Leaving Oban, we steamed past Muck and Eigg, up the Sound of Sleat between Skye and the mainland—like a Suez Canal running between cloud-haunted mountains—and so on to Stornoway in Lews (I saw all this again in September 1920 when I was one of Lord Leverhulme's party at the herring-city). After a glorious but temperate time we returned to Oban on the 25th—when I left the pleasant party and set out by fast train for Edinburgh, where I had promised to dine with (?) Professor Fraser the same evening. But the train started two hours late, and I reached Edinburgh only in time to come in to dinner with the port.

It was to be a great occasion, as Sir William MacGregor and several of the Colonial Premiers were to receive honorary degrees from the university. One of the latter had created a stir by refusing; because, he said, university honours are meant to encourage science, art, and learning, not politics. I heartily concur, and think that our universities often prostitute their honorary degrees in this way; but that did not spoil the fun. I saw many Scotsmen of scientific distinction. Fraser, the worker at antivenene for snake-bite and member of the Indian Plague Commission, E. A. S. Schafer, the physiologist, Andrew Davidson, author of *Hygiene and Diseases of Warm Climates*, who was trying to start a school of tropical medicine at Edinburgh, Drs. Simpson and Littlejohn and, above all, Sir William MacGregor. We received our honours on or after 10 a.m. on Saturday 26 July in the fine MacEwan Hall; and at the lunch afterwards I sat opposite to Sir William at the high table and told him of my approaching visit to Ismailia. He immediately said that he might be able to go with me, and added: "Ross, you and I should change places!"—which was the greatest compliment ever paid to me. David Bruce had received the Cameron Prize in 1899. I foolishly spent all mine in buying a grand new microscope, which I have scarcely ever used.

I was obliged to hurry back to Liverpool because the British Medical Association was holding its annual meeting there in 1902; but do not remember much of what occurred, except that Jones gave a dinner to the Tropical Section on 1 August, at which the Duke of Northumberland, who had become vice-president of our School, was present. After that my family

and myself went for a gorgeous summer holiday to Capel Curig, near Snowdon, in Wales. My long vacations in 1899 and 1901 had been spent in Africa, and this was only my second one at home—absolutely yearned for. Snowdon rose before us beyond two lovely lakes, Moel Siabod [1] towered to left of us, and pearly skies, sudden showers, and glittering sunshine invigorated us. Our great friends, Dr. and Mrs. Walter Steeves (the author of a perfect little book on Bacon and some wise essays and poems) were with us, and suddenly we met on the road Dr. and Mrs. G. H. F. Nuttall, now of Cambridge, and they spent some days with us. One day, after heavy rain, I caught a basket-full of good-sized trout with daddy-longlegs in the tiny stream which runs into the upper lake. We ascended Snowdon (by railway) and returned by the Italian Lake of Llanberis ; and visited Lake Ogwen and gloomy Llyn Idwal, which I am sure Edmund Spenser must have seen on his journeys to and from Ireland, a natural den of dragons. Trifles—like all joys.

But now came the climax of all this long and tedious malaria work—the perfect proof—Ismailia. When I was at the Jenner Institute at the end of April 1902, I received a letter from Prince Auguste d'Arenberg, President of the *Compagnie du Canal Maritime de Suez*, asking for my advice regarding the great increase of malaria during recent years in Ismailia, the headquarters town of the company, which had been founded by Ferdinand de Lesseps and was situated about half-way along the Canal, close to Lake Timsa. M. Chevassus, the representative of the company in London, came to see me, and I finally agreed—not without difficulties raised by the Institute—to go there. After my return to Liverpool, I decided that I must give up some of my vacation in order to be in Ismailia at the beginning of the malaria season in the latter part of September ; and Sir William MacGregor, who was going to Italy, said that he would join me if I could travel to Egypt *via* Brindisi. Accordingly I left Capel Curig (with anger in my heart against yet another journey to a hot country), and departed from London on 12 September, travelling by the P. and O. special train from Dover to Brindisi, where Sir William came aboard the s.s. *Isis*, as arranged ; and we reached Port Said on the 17th. Here we breakfasted at the company's

[1] The pronunciation of this name is indicated in the rhyme which I wrote in the hotel book :

Do you not think it a scrap odd
To live near a mountain called Siabod ?

quarters, where we were almost torn to pieces by swarms of *Stegomyia* mosquitoes, which were evidently breeding freely somewhere in spite of all I had written. We proceeded the same day to Ismailia, where we were housed in the palatial residence of the President himself (who was absent in Paris), and were given princely hospitality. But we scarcely enjoyed it, because every moment we were attacked by legions of mosquitoes—*Stegomyia* in the daytime and *Culex* at night, with a few *Anopheles pharoensis* now and then.

Built a mile or so from the great canal, upon what was originally absolute desert, but was now watered by a fresh-water canal from the Nile, Ismailia had become, in 1902, a well-built, scrupulously clean, little town of about 7,000 inhabitants, of whom 978 were French employees and workmen and their families. The natives occupied a separate quarter, also well kept, and the Europeans were housed as well as was to be expected from the great wealth of the company and the capacity and energy of its president, Prince d'Arenberg (who resided here for many months every year), and of his numerous officers —large mansions, beautiful gardens, perfect roads, under a rainless sky, and not a rubbish heap to be seen or a smell to be smelt, the very pink of good conservancy. Yet here were we, in the house of the president, as in those of his officers (with whom we dined in rotation every night), being almost lifted off our chairs by the mosquitoes, while the malaria which had commenced with 300 cases in 1877 (with the fresh-water canal) had increased to 2,284 cases in 1900! It was even proposed to abandon the town.

There was a considerable staff of officials, belonging to the most intellectual and therefore logical nation in the world. Two years previously they had commenced to drain marshes not far from the town—no good. Early in 1902 they commenced general quinine prophylaxis—very little good. The same year one of the junior medical officers, Dr. A. Pressat, was sent to study the wisdom of the Italians, who, of course, advised more quinine (this officer had now been recalled to meet me). But these gentlemen had never quite understood my basic principle, that where the mosquitoes are there will the larvæ be found also; after discovering a few *Anopheles* larvæ and removing a few pots of water, they observed that the insects remained as numerous as before, and therefore gave up the task as too difficult and sent for me. Well, within two days (as I wrote my wife) Sir William and I (accompanied by a train of officials and workmen carrying creosote and crude petroleum,

which I had brought with me) found numerous larvæ of *A. pharoensis* in a watercress bed, in several small irrigation pools, and in a little shallow marsh of fresh water oozing from the fresh-water canal (there were none in the canal itself owing to the fish), all close to the houses and showed the staff exactly how to deal with them. Next, whence came the *Stegomyiæ* and *Culices*? Not a larva was to be found above ground. There were no rain-water gutters, tubs, cisterns, etc., because there was no rain. They came from marshes miles away, said the officials. But we had remarked that there was an excellent water-carriage drainage-system *without sewers*: where then did the sewage go? Into well-constructed pits called *puits perdus*, situated under each house and hermetically sealed, except for a long ventilation pipe, opening above the roof. Aha! Up went the sealed flagstone over the man-hole of the pit, down went our bucket, and up it came again with a swarming wriggling mass of larvæ! This was where the mosquito-devils bred; and, after hatching, they would fly up the whole length of the ventilation shaft to torment the (rather dull) lords of creation and then back again to lay their eggs. A tumbler full of oil for each pit once a week or so was enough. So much for Ismailia.

On 27 September we went on by launch to Suez, where we found *Anopheles* in a small fresh-water swamp, particularly in hoof-holes in the mud.[1] We were given a morning's fishing in the Red Sea, as a treat, being taken out in a small steamer to a shoal, where we caught large numbers of curious creatures.

We also went to Cairo, which is not malarious, and failed to find *Anopheles* in a marsh there. Sir William wished to buy a genuine scarab, and a dealer produced a large tray of scarabs, all genuine. His Excellency's face grew longer and longer as he examined them. At last he picked one up and said in his gentle manner: "Do you swear that this too is genuine? How very odd. Why, it is made of a kind of stone which is found only in Australia. I did not know that the ancient Egyptians traded with Australia!" We saw the sights and spent an evening at the pyramids, pestered by guides, who were worse than Ismailian mosquitoes; but by this time I had become so sick of wandering and seeing sights that I could

[1] This swamp lay outside the Suez Canal Company's concession, and while we were examining it the local British medical officer came up and, much to our astonishment, roundly attacked us for daring to look at his mosquitoes without his consent (*sic*!). This is the class of person to whom the important duty of sanitation is too often entrusted. If I had been Sir William MacGregor I would have reported the matter officially to the Foreign Office.

hardly look even at them; the pyramids appeared to me like bare-faced plagiarisms of the pictures of them, and I fear I was a poor companion.

I dissected many *Anopheles* at Ismailia in the vain hope of finding *Plasmodia* in them, but showed Dr. Pressat how to perform the little operation. We were treated right royally there. I remember especially Dr. and Mme Dampeirou, M. and Mme C. Dumont at Port-Tewfik, and M. A. Raynaud, *Ingénieur-en-Chef*, a witty Gascon and gourmet. We gave them all a dinner before we left, at which Sir William (who was no gourmet) had ordered the red wine to be iced! I promised them complete relief from malaria in a few weeks. They said that the wine ought to have made me pessimistic instead of optimistic: but I was right. They all came to see us off when we left Port Said for Naples on 29 September.

At Naples we were fumigated against cholera, because it had occurred in Egypt! We saw the sights again, and I bought a small marble copy of the *Aphrodite anadyomene* of the Museum there, to help to furnish our little house in Liverpool. Sir William wished me to stay with him, but I was obliged to hurry back for the autumn term, and left him at the railway station at Rome, and reached home on 6 October.

My report to Prince d'Arenberg was published by our School as *Memoir IX* [71]; and Sir William described the whole work in a letter dated 22 October 1902, to the Secretary of State for the Colonies (printed and published). The officials at Ismailia carried out my instructions exactly, and the victory was complete. The mosquitoes disappeared as if by magic, and next year the French ladies in the town ceased to use mosquito-nets, even for their children. On 2 July 1903 the Canal Company informed me that even in the last quarter of 1902 malaria had "très sensiblement diminué"; that up to date "le nombre des fiévreux paludéens a été considerablement réduit"; and that "les moustiques *culex* ont été supprimes d'une manière presque absolue." All these facts were confirmed in two letters written to me in June and July 1903 by Major R. H. Penton, R.A.M.C., Principal Medical Officer in the Sudan, who had gone to Ismailia to witness the work and wished me to go to the Sudan. The cases of malaria continued to diminish with great rapidity, as shown by the following figures:

Years	1877	1878	1879	1880	1881	1882	1883	1884	1885	1886
Cases	300	400	500	400	450	480	550	900	2000	2300
Years	1887	1888	1889	1890	1891	1892	1893	1894	1895	1896
Cases	1800	1400	1450	1900	2590	2050	1750	1100	1350	1150

Years	1897	1898	1899	1900	1901	1902	1903	1904	1905	1906
Cases	2089	1545	1545	2284	1990	1551	214	90	37	0

After this there were no cases contracted in Ismailia for a number of years. I visited the place again in 1909, and have not followed the subject since. The details will be found given in my *Prevention of Malaria* [91], by H. C. Ross, together with an account of the similar success at Port Said due to E. H. Ross in 1906. In that year the Suez Canal Company published an official pamphlet on the subject called *Suppression du Paludisme à Ismailia* [81]. Dr. Pressat, in a pamphlet (Masson et Cie, Paris, 1905), said that the essential work in the town was done *only by four men* under his direction, and maintained by them. Thus was saved the city of Ferdinand de Lesseps.

The fee which I asked for visiting Ismailia was no less than £100—the fee which a London consultant charges for going to see a single patient in the country; and the Canal Company added my out-of-pocket expenses to it. But from that time onward it subscribed £40 a year to the Liverpool School of Tropical Medicine, although my visit to Ismailia was made independently of the School and during my vacation. Thus do the slaves of science toil for others' benefit.

So here was the end of my seven long years of labour. The world was now in full possession of the facts. But whether it will ever use them or not for its own advantage depends upon itself.

While we were in Ismailia Sir William MacGregor and I anxiously discussed the future of malaria-prevention throughout the Empire. Obviously no general action would be taken unless the central authorities in England insisted upon it and would send out to every one of our possessions in turn men of position and knowledge to instruct local governments and municipalities exactly what to do and how to do it. He therefore himself suggested that on his return to England he would ask the Colonial Office to allow him to give up the Governorship of Lagos (or to second him from it—I have forgotten which) and appoint him Malaria Commissioner for the Empire, with a sanitary engineer and myself as his assistants; and he even hoped that the India Office and the War Office could be got to join in the scheme. It was a grand one, and he was the man for the post; but I confess that my heart rather sank at the prospect, because it would mean endless more labour, innumerable more travails and travels, of the kind from which I had been hoping now to be released; but, of course, I consented. His report to the Colonial Secretary (just referred to)

was obviously written with this object in view. After parting from me he studied malaria-prevention in various places in Italy (mentioned in the same report), and then went to Amsterdam, where sea-water was being let into certain canals near which a small epidemic had been occurring (page 440). Then he came to Liverpool, and on 17 November 1902 gave an admirable address to the African Trade Section of the Liverpool Chamber of Commerce on "The Sanitation of Lagos," which was enthusiastically received by all; Jones gave him the usual banquet the same evening, and the proceedings were published by the Chamber. But, alas! he told me privately that the Colonial Office would not be likely to allow his "Great Scheme," and hinted that there was much jealousy of us in London and a "dead pull" against us somewhere. So there. For the world I was sorry; for myself, relieved.[1] On 28 November he delivered an equally fine lecture on malaria at Glasgow; and shortly after that, I think, returned to Lagos, where his health soon broke down. Ah! the things that might have been but for that dead pull.

In April 1902 I had been asked by Mr. Gerald Christy of the Lecture Agency in London to deliver popular lectures on malaria and mosquitoes, and I gladly consented because I thought that the propagandism would be good for the cause. He arranged everything admirably, and I discoursed, with lantern slides, to the Sunday Lecture Society and the Literary and Philosophical Society in Newcastle in October, and to the Literary and Philosophical Society at Sheffield, the Literary Society at Kingston-on-Thames, the Philosophical Societies at Bradford, Edinburgh, and Aberdeen, and the Armstead Trust at Dundee—all in November 1902. Next year I lectured to the similar societies at Hull, Bristol, Cardiff, and Anerley in February and March—twelve altogether. Kind people gave me hospitality, and the audiences cheered my pictures if not my jokes! On 10, 11, and 12 November 1902 I gave the Harben Lectures of the Institute of Public Health in London, of which Professor W. R. Smith, who had coached me for the Diploma of Public Health in 1889, was Principal. In them I first described my method for the more rapid detection of *Plasmodia* in blood, called the Thick Film Method [72]; it was based upon some work in 1895 (page 168). At Kingston I had the pleasure of dining with Mr. E. H. Man, whose book

[1] He renewed the project when he came home again—letter to me dated 7 April 1904; but now we were to constitute a Commission on Sanitation. Mr. Reeve of Lagos was to be the engineer. Of course there was no result.

on the Andaman Islands (Trübner & Co., 1883) had certainly assisted me in writing my *Child of Ocean*. On 2 December, as already stated, I was made full professor at University College, Liverpool—which was now being converted into a University (charter dated 15 July 1903). On the whole I appear to have been fairly busy during that year of grace, 1902.

But Fortune, not content with giving me so many nice things (with a knock or two) now suddenly conferred upon me out of the blue the greatest and most unexpected of honours. At about noon on Monday, 3 November 1902, while I was finishing my daily lecture in the large lecture-room of the Thompson-Yates Laboratories at the University of Liverpool, Boyce brought in Sir Matthew Nathan, the Governor of the Gold Coast. After talking to them for a few minutes I went to my laboratory close by for something, and found the following registered letter there:

KAROLINSKA INSTITUTET

MR. RONALD ROSS,
LIVERPOOL. STOCKHOLM *d.* 30 *Okt.* 1902.

DEAR SIR,

I have the honor to inform you that the Professorial Staff of the Caroline medico-surgical Institute in Stockholm has this day decided to awarde to you the medical Nobelprize of this year in amount of 141,846 sweedish "kroner" (about 7.880£) for your work upon the Malaria. The distribution of the Nobelprizes will take place here in Stockholm on the 10th of December this year. Until this day your nomination as prize-winner is a secret. I beg you to inform me—if possible with returning mail—wether you will meat here personally on this day. I may further draw your attention on the 89 of the code of Statutes in expecting as soon as possible the information, wether you will give the said lecture one of the days after the 10th of December, or later.

I hand you a copy of the prescriptions, which the administration of the foundation has given concerning the identification of the prize-winners.

Respectfully yours,
K. A. B. MÖRNER,
Rector of the Caroline Institute.

Address:
PROFESSOR, COUNT K. A. H. MÖRNER,
HANDTVERKAREGATAN 3 STOCKHOLM
SWEEDEN.

This was not only a very great honour but—a reality!

I was so astonished and delighted at the very first sentence of the letter that I returned to Sir Matthew and to Boyce crying: "I have got the Nobel Prize!" Then, on reading the letter through, I found the clause enjoining secrecy—but, of course, both of my companions swore that they would not reveal the secret for worlds. Alas, as I said in my "Deformed Transformed":

A secret like vase once crack'd is broken.

About an hour later I went to one of the clubs in Liverpool where Jones was giving a lunch to Sir Matthew. On the way I telegraphed the good news to my wife—"strictly private." Directly I entered the club numbers of people came up to me with congratulations, and the secret appeared in the papers a day or two later![1] The same evening I went to lecture at Sheffield.

The Nobel Prizes were founded by the will of Alfred Nobel, the "dynamite-king," who was born at Stockholm on 21 October 1833 and died on 10 December 1896. By his testament of 27 November 1895 he left a large part of his great wealth for the purpose of founding five great annual prizes amounting to about £7,800 each, for physics, chemistry, literature, medicine, and peace-propagandism. The first three are awarded by Swedish Academies, and the Medical Prize by the Caroline Institute of Stockholm, and the Peace-Prize by a committee of the Storthing at Christiania. Special funds were allotted to pay committees and assessors for the careful selection of candidates, which, I was told, required six months' anxious deliberation, because the prizes are open to the whole world. The first prizes were distributed on Nobel Day 1901 to W. H. Röntgen, J. H. Van't Hoff, Sully Prudhomme, E. von Behring, for the first subjects in the order given, and the Peace-Prize to J. H. Dunant and F. Passy. In 1902 they were given to H. A. Lorentz and P. Zeeman for physics, E. Fischer for Chemistry, T. Mommsen for literature, me for medicine, and E. Ducommun and C. A. Gobat for peace.

On 4 December my wife and I left Liverpool with Miss M.

[1] I mention these details because someone in Stockholm was subsequently blamed for the leakage. Of course I wrote at once to the Nobel Committee and laid the blame entirely on myself. At the same time it would have been impossible anyway to withhold the news till Nobel Day, the 10 December, as the committee wished, because the prize-winners must obtain leave and take their tickets for Stockholm. Personally I hate secrets of all kinds—and evidently cannot keep them!

Ribbing, daughter of Seved Ribbing, Professor of Medicine at Lund. He had been our first student at Liverpool (in spite of his years, page 376), and she was a charming and capable young lady who happened to be returning home from England to Lund at the same time. We travelled via London and Harwich to Esbjerg on the west coast of Denmark (a tempestuous voyage), and then by train to Copenhagen, where we slept at the Hotel Phœnix on the 6th—my wife and I were nearly suffocated by our mismanagement of the great china Swedish stove in our room. Next day in brilliant, windless, but icy weather we took the ferry to Malmö, and then, by a short train journey, reached Lund, where Ribbing, his wife, son, and younger daughter entertained us. The Swedes say that the British are the best-looking of nations, but I should return the compliment in favour of the Swedes—tall, blond, debonair, formal with the formality that makes manners, simple, kindly, and yet accomplished. The young men on being introduced to one bow three times, each bow deeper than the previous one; and the young ladies are all Astrellas! They gave us Christmas hospitality—which meant that one continued eating something or other from midday to midnight, with occasional interludes of pretty Swedish songs and really serious meals! But Ribbing and Professor Karl Furst explained that they did not dare to treat us fully as we deserved because we were still *incognito*! We left by night-train on 8 December, and I will describe what occurred from a record which I wrote after our return home.

We arrived at Stockholm with Ribbing on the 9th. The country was covered with a thin layer of snow, the sun shone brilliantly, and though the temperature was many degrees below zero we never felt cold for a moment, owing to the admirable manner in which the Swedes construct and warm their railway carriages and houses—far better than we do. In that still, bright, and frozen air the beautiful city looked like a flower embedded in an icicle—another world from that of Madras, Lagos, Freetown, and Ismailia. Fine rooms had been taken for us in the Grand Hotel, and we were visited at once (though still *incognito*) by members of the Nobel Medical Committee of the Caroline Institute, Professor Count K. A. H. Mörner, President; Professor K. O. Medin; Professor E. Almquist; Professor K. Sundberg; and Professor E. Holmgren. Mörner was a delightful man, ruddy, fair, grave but humorous, who spoke and wrote English well, became my life-long friend, and often visited England (I motored him through North

Wales in 1912). Almquist was the man, I now learnt, who had written to me a year previously on the priority question (page 456). I had come without a top-hat, but Mörner told me that I must buy one at once for Stockholm !

The ceremony of the Nobel Hogtidstag commenced at 7.30 p.m. on Wednesday 10 December. Professors Fischer, Lorentz, and myself were taken possession of by the presidents of our respective Nobel committees, who showed us into the great crowded hall of the Academy of Music and placed us next to the ambassadors in the front row of seats " facing the music." Presently King Oscar II—tall, fair, benign—accompanied by Princess Ingeborg, the Crown Prince, Prince Charles, Prince Eugene, Prince Gustavus Adolphus, and their suites filed in and seated themselves in the middle of the front row, while Emmanuel Nobel (representing the Nobel family) and many high functionaries accompanied them. The ceremony commenced with the overture of F. Berwald's opera, " Estrella de Soria," and the oration from the prolocutor of the Nobel Direction. Then came the magnificent Horr oss Svea (Hear us Svea), rendered by a male choir seated in front of the King, followed by the prize-giving. First an orator declaimed the merits of Professor Lorentz (one of the greatest physicists) and, descending and taking him by the hand, introduced him to the King, who presented him with the Nobel medal and album and shook hands with him. Next, as Zeeman and Mommsen could not attend, Fischer received his prize in a similar manner ; and then Professor Grëfve Mörner, Rector of the Caroline Institute, announced my merits in Swedish (almost as eloquently as I have done in this book), and concluded by saying in English :

" Professor Ronald Ross, in announcing that the professorial staff of the Caroline Medico-Chirurgical Institute has decided to award to you the Medical Nobel Prize of this year on account of your work on malaria, in the name of the said Institute I congratulate you on your investigations. By your discoveries you have revealed the mysteries of malaria. You have enriched science with facts of great biological interest and of the very greatest medical importance. You have founded the work of preventing malaria, this veritable scourge of many countries."

The albums and medals which the King gave us were exquisite

works of art designed and executed by Eric Lindberg; and the cheque was hidden unobtrusively within the former. The prizes of Mommsen and Zeemann were delivered to the Ministers of their respective countries; and the ceremony closed with music by Lindblad and Beethoven and the National Anthem. It was impressive to see the good King standing facing his people with bowed head as they sang that fine piece in his honour.

The Banquet followed immediately at about 9 p.m. and was presided over by the Crown Prince and his brothers. They sat together in the middle of the high table with the Ambassadors and then ourselves next to them; and, as usual the toasts came in with the dishes. The Crown Prince touched his glass to each of us in turn (in reversed order from that at the Ceremony) and we rose to our feet to respond; after which the same orators briefly proposed our health, and myself, Fischer, and Lorentz replied—not always briefly! My speech was the shortest, and here it is.

"Your Royal Highnesses, Grëfve Mörner and Gentlemen, I beg to thank you for the very great honour you have done me in drinking to my health this evening: and you Professor Mörner for the eloquent and flattering terms in which you proposed the toast. I beg to accept the honour, not only for myself, but for all those who have laboured so long at the important subject of malaria. Permit me at this auspicious moment to mention the names of some of those to whom humanity owes so much, but who have not always been as fortunate as myself in receiving reward for their labours. I will begin with the great name of Laveran, who more than twenty years ago discovered the cause of malaria and created a new branch of science—Laveran, that true man of science who has honoured me by permitting me to call him my master. I will mention next the names of Golgi, that most distinguished Italian; of Danilewsky, of Marchiafava and Celli, of Kelsch, of Mannaberg, of Bignami, Romanowsky, Sakharoff, Canalis, Bastianelli, Dionisi, Vandyke Carter, the two Plehns, Ziemann, Thayer, and not least MacCallum, who, with a host of others no less meritorious, consolidated the discovery of Laveran. Turning now to the subject of malaria and mosquitoes, I must first mention those who created the hypothesis, namely King

in America, Koch in Germany, Laveran in France, and particularly Manson in England, whose profound induction formed the basis of my own humble endeavours, and whom also I shall always esteem one of my masters. Now permit me the honour of naming those who in all parts of the world confirmed and amplified those elements of the truth which I had found in India—the great Koch and his German colleagues; Bignami, Bastianelli and Celli in Italy; Daniels, Stephens, Christophers, Ziemann, Annett, Dutton, Elliot, Van der Scheer, Van Beerlekom, Manson and his son, Fearnside, James, Nuttall, Austen, Theobald, Howard and many others. Nor let us by any means forget those who are endeavouring to turn these discoveries to practical account for the saving of human life on a large scale, particularly Koch, Sir William MacGregor, Celli, Logan Taylor and Gorgas; and, not least, Sir Alfred Jones and those merchants of London and Liverpool who are spending their money freely for the same great cause.

"In conclusion, Gentlemen, I hope you will permit me to utter a personal note. I cannot help comparing the present moment with that when, seven years ago, I commenced the researches for which you have to-day given me such great honour. I cannot help remembering the dingy little military hospital, the old cracked microscope and the medicine bottles which constituted all the laboratory and apparatus which I possessed for the purpose of attacking one of the most redoubtable of scientific problems. To-day I have received in this most beautiful capital of the north, the most distinguished of all scientific honours from the hand of your King himself. Gentlemen, I can do no more than thank you."

Meanwhile many of the ladies of Stockholm, headed by Grevvina Mörner, gave a similar banquet to my wife and to Frau Lorentz, at which they also had to exhibit their eloquence —much to the dismay of one of them—of course everyone in Stockholm understands English and German. Followed a large but informal gathering at which we were introduced to many delightful people and to a copious supper, commencing with the Swedish smörgos-board, and accompanied (as I observed from a distance) with a Scots fluid taken mixed with soda-water. We did not get to bed until 3 a.m. Next day's newspapers were filled with the doings.

Now followed a series of entertainments. On 11 December we were introduced to the Academy of Sciences, where we heard a paper in French on "Finite Differences." We delivered lectures next day to crowded audiences, and were entertained by Count Mörner, Professor Retzius, Dr. Edgren, and their ladies, and by the British Minister. I was also received by the Medical Society, where the president, Dr. K. Malmsten, and younger members marched me round and round a long table singing songs to an accompaniment of tankards. We were shown the beautiful outskirts of the city; and when we three "laureates" paid our respects to the King (on the 13th), he himself took us round his house (as he called it) and showed us the treasures it contained. I remember also Svante Arrhenius, the great physicist, Sir Sven Hedin, the traveller, Emmanuel Nobel, Professor S. O. Pettersson, and many others—not least Mme Retzius and Mme Edgren; we entertained many of them at dinner before we left. At last (Sunday, the 14th) the sad time came to call round and say farewell to our many friends; but when we went to the station there they all were in a body! We stayed again next day at Lund and, after surviving our incessant stream of Christmas dainties, were conducted home to our hotel late at night by a band of students singing (not bawling) part-songs. On the 16th Miss Ribbing led us back to Copenhagen, showed us the sights, and put us in the train for home, via Flushing.

A full account of the ceremonies and the speeches will be found in the series of publications called Les Priz Nobel, issued by the Nobel Committee (Norstedt, Stockholm). My Nobel Lecture entitled "Researches on Malaria," giving a history of my work and occupying eighty-nine pages, is contained in the number for 1902 (published in 1905); but of course the spoken lecture was only an outline, and the written work was not completed till 1904. It was then copied in the *Journal Royal Army Medical Corps*, and translated into Italian by F. Maiocco, and into German by C. Schilling [78].

In 1910 I went again to Stockholm to receive the honorary degree of M.D. from the Caroline Institute, was present at the Nobel prize-giving of that year, and saw with delight all my old friends once more.

On 22 December 1902 the Lord Mayor and Lady Mayoress of Liverpool (Councillor and Mrs. W. Watson Rutherford) gave me a great banquet, followed by a reception, at which my wife was presented with a bouquet, while my children watched the scene from a balcony. The Lord Mayor then sent a tele-

gram to King Oscar, thanking him on behalf of the City of Liverpool; and another to King Edward, both of whom graciously replied. On 24 January 1903, the University College Association of Liverpool gave me yet another feast, to which Dr. Manson came, all the way from London; and, lastly, on 9 February, I entertained fifty of my friends at the University Club, in thanks for their kindness.

CHAPTER XXV

PANAMA, GREECE, MAURITIUS, CYPRUS, THE WAR. 1903–1922

THE story of tropical medicine now broadens into so great a flood that no single man can hope to ken all the deeps and the shallows of it. Events followed fast. The parasite of kala-azar, which I had failed to find in 1898, was discovered [74]. Sleeping sickness was shown to be due to trypanosomes and to be carried by tsetse flies—as Bruce had proved years previously for the nagana of cattle [18]. Theobald brought out his great monograph on mosquitoes [63]. The spirochætes which produce relapsing fevers were found to be inoculated by ticks. The embryos of hook-worms were proved to enter us through the skin of the feet as easily as sand-eels dive into sand. Numerous skin-diseases were accurately studied. Mediterranean fever was traced to infected goats' milk. Death-bearing insects were classified everywhere. Haffkine's vaccines against cholera and plague and a similar one against typhoid were brought into general use; and, greatest of all, the arch-pestilence, plague, supposed to be sent by Heaven for our punishment, was traced to the rat-flea! Mean little matters these, some will say; but I maintain that they are among the greatest events in the history of mankind.[1] This book is not, however, a record of tropical medicine, and though I possess boxes full of papers on these interesting developments, I must now jump through the years and wind up my personal story with some brief notes on less important events which occurred after 1902—that is, during the last twenty years.

Owing partly to my duties at Liverpool and partly to the reasons given at the end of Chapter XXI, I was never again able to throw my heart into microscopical work, though I had planned several important investigations—few of which have

[1] Dining once with the Rev. T. W. M. Lund of Liverpool—that most perfect preacher—I happened to be describing one of these "germs" when the Rev. J. Watson (Ian Maclaren) asked me, "Do you really think these things are of any importance?" "Yes," I answered, "more important than, say, the battle of Waterloo." After thinking awhile he said, "Now please tell me why." "Because," I replied, "Napoleon is said to have killed a million people during his life; but anyone of these parasites probably kills that number every year."

been commenced yet by anyone. The stories of even such great men as Newton and Huxley show that intense mental labour is apt to be followed by reaction, probably due to physical causes; moreover I remained for years condemned to propagandism and polemics, and was engaged in an enormous correspondence all over the world. So far as I know, Manson also did little more microscopic work; but he created some more hypotheses, as that *Filaria perstans* may be connected with sleeping sickness, and monkeys with yellow fever; but these proved less successful than his great induction regarding malaria had been. The solution of the malaria problem is still attributed to many people; but I see that my humble name, like that of Themistocles after the battle of Salamis, always comes in *second*—King and Ross, Koch and Ross, Manson and Ross, Grassi and Ross, Bignami and Ross, Sambon and Ross, and so on!

The reader will ask what happened ultimately to our sanitary efforts in West Africa. The transfer of my Tropical Sanitation Fund to our School Committee early in 1902 (before I went to the Jenner Institute) broke the continuity of my design described on page 439; but we kept Logan Taylor at Freetown for the appointed year and did all we had designed to do. We had greatly reduced the mosquitoes, cleaned up the town, and delivered our "object lesson"; but whether the local authorities would continue the work depended, as we had warned them, entirely upon themselves. They showed no sign of doing so; and in a paper which summarised his work and conclusions (*British Medical Journal*, 15 September 1902) Taylor said clearly: "Even after one year's free scavenging of their town, the Freetown authorities do not seem in the least inclined to begin and do it for themselves." Mr. Coats's money being now nearly expended, we sent on Logan Taylor to the Gold Coast, where the Governor, Sir Matthew Nathan, wanted him (page 457). When Mr. Swanzy's money and the residue of Mr. Coats's fund were exhausted, we recalled Taylor to England (April 1903), and closed the expedition. But I was meditating new journeys to Lagos and the Gold Coast, when the following event happened at the Malaria Committee of the Royal Society.

That committee had greatly disappointed us in 1898 (page 317), and again in 1900 (page 421). It seemed to be more interested in parasitological and entomological details than in the application of them to practical sanitation, and I had even suggested my resignation of it to Lord Lister, but remained at his wish (letter of 24 January 1903). Boyce (who became a Fellow in

1902) was now also a member of it ; and we attended a meeting together on 27 May 1903, on some matter which I have forgotten. Suddenly a letter from the Colonial Office, which was not on our agenda paper, was read out to us. It was an attack on Logan Taylor and myself by the Sierra Leone authorities, who praised themselves, declared we had done nothing, and now demanded a commission of inquiry on our work (although we had left the place a year previously), and the Colonial Office asked the advice of the Malaria Committee as to whether such a commission should be appointed. The Chairman, without waiting for my reply, moved that it should be appointed, and he even refused to hear my answer to the Sierra Leone attack because, he said, it would have been irrelevant. Boyce and I objected on a point of order, and the meeting was postponed to 17 June 1903. There, one of the members at once proposed the original resolution ; and, without listening to what I had to say, the committee decided in favour of the commission of inquiry.

Of course, as everyone acquainted with practical sanitation would know, the Sierra Leone proposal was not only impossible but absurd and dangerous. No commission could possibly gauge the value of our work a year after it had been discontinued—a year after the local authorities had allowed the town to revert to its original condition. The oil which we had put on the puddles would have evaporated, the channels we had cut or cleared in the drains and gutters would have become choked again with weeds and stones, the backyards would have been filled up again with mosquito-breeding rubbish. The doctors there had kept no reliable statistics and had refused to help us by taking " spleen-rates," so that the effect of our work on the public health could not be estimated. In fact the proposal was mere whitewash. We should be condemned if we were to object to it ; and, if not, the commission would enjoy the hospitality of Freetown, learn all about our misdoings in our absence, and be in the position of a judge and jury who dine with the plaintiff before the trial. Moreover it was evident that the whole matter had largely been arranged behind the scenes and the members of the commission probably chosen ; and I gathered that not one of them knew anything whatever about tropical sanitation. The great danger was that they would end by condemning mosquito-reduction, which would then probably be debarred in all the colonies ; and would finish by advising—what costs government nothing—other people to take quinine for the rest of their lives. That would

be the death, not only of my efforts, but of those of Sir William MacGregor and Sir Matthew Nathan and others. So I went home to Liverpool and wrote a strong letter (19 June) direct to Mr. Chamberlain, and another still stronger one, dated 9 July, with the result that, after some face-saving correspondence, the proposed commission was quashed.[1] Lord Lister was not present at these meetings and, I believe, knew little about them. Thus ended my Tropical Sanitation Fund and my visits to West Africa, and this is all the thanks that Logan Taylor and I ever received from that (very) dark Continent.

The fact is that mosquito-reduction was very unpopular among officials; governors disliked the expense and doctors the trouble. They combined, not against mosquitoes, but against me; and all my old adversaries, the blue-mistics, the Grassians, and the vacuum-theorists, helped them.[2] For years I was freely and frequently demolished in the Press; and, alas! even some of those who I thought were my friends curried favour with the authorities at my expense. On no less than three occasions was I obliged to go to my lawyer's for defence against the bitter attacks made on me. Such is the lot of the would-be reformer; but after all I got off lightly compared with Colonel W. G. King (page 243) and Mr. W. M. W. Haffkine. The latter had conferred two great benefits on the people of India—his vaccines against cholera and plague; but, owing to an accidental contamination of some tubes in 1902, a number of people died of tetanus, and he himself was blamed because, it was said, he had neglected certain precautions. He proved easily that the accident was not due to any fault of his, but was nevertheless set upon by the usual class of people and nearly ruined. A number of us (especially Professor W. J. Simpson) had to fight hard for him in the Press before the Indian Government's decision was reversed (see *Lancet*, 2 February 1907, and my hot letters in *The Times* of 15 March, 13 April, 1 June, and 29 July 1907—the last drafted by me and signed by ten leading pathologists).

[1] The commission would probably have cost as much money as would have sufficed to keep all the West African towns clean for several years—an example of our British infatuation for commissions of alleged experts to advise on and so delay reforms which are already obviously sound and necessary. Sir William MacGregor thought that the attack was a personal one against myself (page 475), and certainly some of our opponents were subsequently given honours, and one an appointment at the Colonial Office. See also the *Sierra Leone Medical Report* for 1902, and *The British Medical Journal*, 6 June 1903.

[2] At the same time, most of them were making "quite a good thing" out of my work.

The amazing truth is that not only mosquito-reduction bu all sanitary advances are unpopular, and for the same reason Sanitation is the world's business, and therefore nobody' business. Reformers are frequently treated exactly as show in Ibsen's comedy, "An Enemy of the People"; and th rewards of the medical profession are given not to them, bu to persons whose work is infinitely less important. Lastly even the most brilliant results of sanitation are of a negativ character; not something tangible, like a new post-offic school, or hospital, but a mere absence of what few peop ever see—illness and death. Gorgas told me in 1904 tha after he had cleared Havana an American official said to him "I don't know why you make such a talk about mosquito and malaria: I have been here a week and have seen ver little of either"!

On 29 October 1903 Mr. and Mrs. Chamberlain paid a su prise visit to Liverpool, and Lord Derby (the sixteenth ear asked Jones, Boyce, and myself to meet them privately a lunch at the Adelphi Hotel. The great minister, with h cigar and eyeglass, was a plagiarism of his caricatures, ar acidly disputed some of Jones's schemes for West Africa, un the latter said laughing: "Well, Mr. Chamberlain, everyo knows that you snub us all but that Mrs. Chamberlain smoot us down again!" He was complimentary about our sanita work (possibly to make amends for Sierra Leone), but wh I pressed him regarding sanitary commissioners for We Africa (page 436) he said that he was not going to set spies ov his African officials (*sic*)! Of course inspection is necessa in all good management, and we have inspectors of navi armies, schools, and municipal sanitation, down to sho walkers and headmen of working gangs, all of whom mig be described as spies. When Jones and Boyce hinted that t only way to get anti-malaria work properly done in Afri was to send me out there in an executive capacity under t Colonial Office, Chamberlain changed the subject; and I co cluded that further efforts were hopeless. It did not matt to me personally how many people died there, and I h already done as much as a private individual could do in t line and as—well, as my countrymen deserved. Sir Willi MacGregor entirely agreed with me when I told him la of the interview—which had no result whatever. Thus it w that the glory ultimately passed to Gorgas and the America

But we were not defeated yet. On 16 October 1903 lectured at Anderson's College, Glasgow, and was given t

honour of a great official banquet afterwards by the Lord Provost, Sir John Ure Primrose, and the Corporation. On 10 November I lectured before the Royal Colonial Institute in London (dinner afterwards); on 12 November, to the Royal Engineers at Chatham; and on 2 December, to the Clinical Society of Newcastle (another banquet!). At all these lectures I put my case again, and even tried to get up a joint petition to Government. I also lectured before the congress of the Sanitary Institute at Bradford in July 1903, and sent papers to a medical congress in Brussels and to the British Medical Association; and, as already stated (page 475), Sir William MacGregor returned to England next year and renewed his efforts to form an imperial sanitary commission. But all this availed nothing against the "dead pull." He was transferred to Newfoundland on account of his health in 1904.

Meanwhile most of the doctors, especially in India, were preaching the impossibility of reducing mosquitoes anywhere, because, they said, if we cleared them from one spot, they would rush in from outside like gas into a vacuum. I therefore made a mathematical study of the question, which resolved itself into this one: If a million mosquitoes be liberated from a box in one spot and be allowed to wander freely in all directions under equal conditions until they die, then how far from the box will their dead bodies be found? It was a problem in the Law of Chances, and after much labour (I could get no mathematician to assist me) I found a tentative solution: that their numbers would be greatest near the box and would diminish as a function of the distance away from it. I called this the law of Random Migration; and my paper, which showed by means of simple proofs and diagrams the absurdities of the vacuum-theorists, was read at St. Louis in September 1904 and was published in 1905 [8]. But as the law of Random Migration had several wide applications, even to the Theory of Evolution, I asked Professor Karl Pearson early in 1904 to give me a better mathematical solution than I had found. He gave it in a pamphlet called *A Mathematical Theory of Random Migration* [80], and confirmed my results by means of much more elaborate analysis.[1]

But all this was above the heads of our opponents, and they soon invented yet another war-cry to avoid their obligations.

[1] The solution which I had asked for was not sent to me before this paper appeared. The paper does not mention my previous article. It refers to my original request for assistance, but dates this a year later than it was made. Mr. W. Stott sent me another solution still later. I have not studied either solution very carefully.

Nothing was to be attempted against mosquitoes until the native populations were sufficiently educated—which will be, never! Of course mosquito-reduction is a *State* service, a part of ordinary town-conservancy; and one might as well postpone street-sweeping, latrine-cleansing, and the making of sewers indefinitely until the millennium. Even Manson joined in this cry (see, for instance, *The British Medical Journal*, 10 August 1907, page 388), which is still repeated by people who have no experience of practical hygiene.

Consolation for India and West Africa began, however, to arrive in 1903–4, apart from Ismailia. Malcolm Watson had cleared Klang and Port Swettenham [91, 99]; Hongkong was much improved [91]; Dr. Andrew Balfour wrote me (8 February 1904) that he had commenced mosquito-reduction at Khartum [91]; and Gorgas had scored a great success at Havana [91]. The last named went to the Medical Congress held at Cairo on 14 December 1902 (to which I also had contributed a paper), but was not impressed with Egyptian activity. He proposed to visit me in Liverpool on his way home, but was prevented for the reasons given in the following interesting letter:

8 Rue Marbeuf, Paris,
January 27th, 1903.

My dear Major Ross,

I have just received a cablegram from the War Department directing me to return to Washington immediately. So I will have to forego the very great pleasure of my visit to Liverpool this trip. This is all the more a disappointment after your very cordial letter of the 23rd. I sail on the mail steamer, the 31st.

I have seen a good deal of Europe this trip but missed what I was most anxious to see, England. I am an American who looks upon England as mother-country and I wished much to see the country from which my forefathers had come.

I rather think my recall has something to do with the Panama Canal. An intimation has been given me that I was to go down there as sanitary officer when work commenced. If such is the case I will have a broad field for anti-malarial work and I hope to be able to again show what an incalculable boon to mankind your discoveries have been. I know of no place where malaria has caused such havoc as among the labourers at Panama both in building the railroad and in the attempts

PLATE VIII.

MALCOLM WATSON

to build the canal. So far it has been the greatest difficulty in the building of the canal. If we can materially lessen it, or entirely get rid of it, it will be a very useful object lesson. Allow me again, my dear Major, to thank you for your very cordial invitations; and to express the hope that my meeting you has merely been deferred.

Very sincerely yours,
W. C. Gorgas.

I had arranged to go in 1904 to the great International Congress of Arts and Sciences to be held at the World's Fair in St. Louis, U.S.A.; and after I informed Gorgas of this fact an invitation reached me, either from him or from Surgeon-General Walter Wyman, head of the American Public-Health and Marine-Hospital Service (original letter lost), to go on to Panama afterwards. The Congress was to give its European visitors £100 each for expenses, and the Isthmian Canal Commission offered me (letter 6 August 1904 from Gorgas) free passages from New York to Panama and return. Consequently I left Liverpool in the *Lucania* on Saturday 10 September 1904, reached New York on 17th, and St. Louis on 19th, where we were all housed in the World's Fair, a city of magnificent white palaces with statues and fountains, all made of lath and plaster. I was really to have opened the whole discussion on Pathology, but was led to believe that I might choose my own subject, and hence I read the mathematical paper just mentioned to hundreds of disappointed doctors who did not understand a word I said !—my apologies to them herewith. There I met numbers of distinguished Americans, Osler, Councilman, and Thayer among others, and was welcomed by an American chemist as the author of *Blowpipe Chemistry* (my uncle, page 6). We drank beer in Swiss chalets and tea in Chinese pagodas; but after a few days I escaped alive with William Osler, in order to stay with him at Baltimore. On nearing that city I fell asleep in my chair in the train, but was awakened by Osler shouting "Simla" in my ear, to the amusement of the other passengers. I had my revenge. We were bitten to death by mosquitoes in his house, and he, great man, though the recognised authority on mosquitoes in Baltimore, could not find their larvæ. I found them at once in his own back-yard, and told a reporter; and the affair appeared next in the newspaper under the headlines, if I remember aright, "Dr. Osler's Eye Wiped." I saw Washington and the great library

there, went with Dr. Musser to Philadelphia, and embarked on the s.s. *Advance* for Panama on 27 September.

Unfortunately Gorgas was then on leave and could not voyage out with me; but he and Mr. Henry Clay Weeks, Secretary of the American Mosquito Extermination Society, came on board to see me off (photograph herewith). Gorgas was a spare resolute man of the best type, and we cordially discussed everything together for some hours. He was astonished to hear of the " vacuum-theorists " and of the opposition in India and Africa, but said that Panama would defeat it. We had a fine voyage, passed through the straits between Cuba and St. Domingo (the scene of my romance, *The Spirit of Storm*), and reached Colon on 4 October—a settlement on a sandbank with a brisk breeze always blowing a bright foaming ripple towards it. To Panama the same day by a ramshackle train—a windless, hot look-out upon the Pacific, always haunted by heavy clouds and daily rain and surrounded by masses of vegetation. It was pitiful to see the parks of rotting cranes and locomotives left by the French in consequence of the yellow fever and malaria, the latter bred from innumerable stagnant pools between innumerable small crumbling hills. I was housed in the quarters of the medical staff, and that night all our bungalows were robbed by a gang of American cracksmen while we slept, and I lost the £100 which the St. Louis Congress had given me and had to subsist on local charity during the remainder of my stay! But I saw many of the most distinguished " yellow-fever men "—H. R. Carter, who discovered " extrinsic incubation," J. W. Ross, T. C. Lyster, L. Balch: and Mr. J. A. Le Prince showed me what was being done. I dined with General Davis, who commanded, saw the Canal being dug, lectured to a perspiring congregation on 10 October, and left for home after many farewells on the 12th. There was little advice to give because everyone knew his work already. On leaving Colon we came into the tail of a cyclone, and I thought that the old *Advance* would have gone under; but we arrived safely at New York on Saturday 21 October. I had only one dollar in my pocket and no hat, but the Waldorf Astor Hotel took me in on trust because I fortunately had a note of credit on a New York bank. Dr. A. H. Doty, the Port Health Officer, who had himself done much work against mosquitoes in the New Jersey marshes, planned to give me a triumphant send off; so when the s.s. *Arabic* was working out he circled round us in his port steamer, flags up, and trumpeting incessantly—till the passengers thought there must be a dis-

WILLIAM CRAWFORD GORGAS, RONALD ROSS, HENRY CLAYE WEEKS, ON BOARD THE S.S. *ADVANCE* AT NEW YORK ON 27 SEPTEMBER 1904.

4921

guised criminal, or duke, or even politician on board! We were home on 29 October. Everyone knows how magnificently Gorgas maintained the health of the Canal Zone from that time till the great undertaking was finished. If I had had my way the same thing would have happened in every British colony.

Ever since 1897 I had had little time for literature; but before going to Panama I collected the old notebooks containing my poem *In Exile* (page 226), and, on that stormy voyage home from Colon, busied myself arranging the "sonnetelles" so as to make them presentable for printing. This was, I think, the first time I had ever read them since they were written; and I now thought myself a prophet!

> Though in these depths of night
> Deep-dungeon'd I was hurl'd,
> Thou sentest me a light
> Wherewith to mend the world.

and

> I know this little thing
> A myriad men will save.

Having more leisure now, I continued this work after my return, and the poem (by "R. R.") was ultimately printed for me privately in February 1906 [112], after rejection by three publishers, but was not published till 1910 (page 506). At the same time I recommenced the study of my system of operative algebra and found a beautiful method for solving algebraic equations by "operative division." On 20 December 1904 the high honour of Doctor of Science was conferred upon me by Trinity College, Dublin. I stayed there at the Observatory (where Hamilton had lived) with my friend, C. J. Joly (page 456), who was much taken with my method. The result was that my paper, called *Verb-Functions*, was published in the proceedings of *The Royal Irish Academy*, vol. xxv. A.3, April 1905 [102–105].

Alas, Joly died suddenly on 3 January 1906.

On 12 January 1905 Princess Christian and Mr. and Mrs. Joseph Chamberlain visited Liverpool, partly to see our School. In the afternoon I lectured at St. George's Hall on the Progress of Tropical Medicine before the Princess, Lord and Lady Derby, Mr. and Mrs. Chamberlain, and a large concourse; and in the evening my wife and I dined at Knowsley. I sat next to the Princess, who was much interested in our work, and became Patron of our School. Her eldest son, Prince

Christian Victor, had died of alleged malaria in 1900. The lecture was published in 1905 [79].

In the year 1906 a chair of protozoology was established in London. I thought I ought to apply for it, though the salary was small. The committee rejected me, but asked two foreign zoologists, one of whom had done little, while the other had made a colossal mistake in connection with the subject; and when these gentlemen declined the post they appointed an English zoologist. A little later, the same people asked me to go to Uganda to study trypanosomes—a branch of that science. I refused—saying that as the zoologists had won the prize they should now begin to do the work.

My next excursion was to Greece in 1906. The Lake Copais Company of London owned 60,000 acres in that famous haunt of eels and wished me to advise them regarding the malaria there. My wife and I were to have free tickets and out-of-pocket expenses, to which the company kindly added £50. We left Liverpool on Friday 18 May 1906 and, travelling by *The Oriental Express* from Ostend, stayed a day and a half at Vienna, where we were entertained by our friends Dr. Alfred Fröhlich (the physiologist) and his wife, and by Dr. Julius Mannaberg (page 456), and saw the aged Emperor and his palace. Proceeding via Trieste by an Austrian steamer to Patras, we traversed the route which I had seen in 1895, and was to see again from a French destroyer, after being duly torpedoed, in 1917. Then by train the same day to Athens.

What a journey—every mile a haunt of history. Opposite to us across the Corinthian Gulf were Messolonghi, where Byron died, and then Delphi, with Parnassus and Helicon sparkling in the cloudless sky; and we ran through Corinth, Megara, and Eleusis, glimpsing the Saronic Gulf, to the Olive City, which we reached in the evening (25 May). There we were entertained by Mr. B. Steele, the agent of the company, in his house in Homer Street, at the feet of Lycabettus. Why does not every tourist in creation come here? We visited the Acropolis by the setting sun, when the perfect mountains were of an olive green and the sea deep indigo blue; and when the stars came forth the sky was not blue but imperial purple. Here fitly dwelt the Goddess of Science—unknown to baser peoples.

For three days we saw the sights, called on the Ambassador, Sir Francis Elliott, and on the President and Secretary of the Greek Antimalaria League, my friend Dr. C. Savas, Professor of Hygiene at Athens and Physician to the King, and Dr. J. P.

Cardamatis, a diligent worker at malaria. Then on 28 May, accompanied by the latter and by Mr. Steele, we took the train past Tanagra and Thebes and the Sphingion to the Copaic Plain—formerly a lake, now a great cultivated expanse dotted with villages and farms, and surrounded by Helicon and other mountains, with Parnassus gleaming in the distance—just as my prophetic eye saw it years before (page 48). We lived in the company's house at the northern foot of Helicon in a beautiful grove—which is described in my *Setting Sun* [116] under the figure of Pallas Athene's garden :

> Here in Her deep and dusky dell
> The silent cypress groweth well.
> Along the lawns of level grass
> The tinkling streamlets pause and pass
> Pierian, nor thy Hippocrene
> More bubbling-beauteous to the scene.
> To their low notes the nightingale
> Enlayeth her long-linkèd wail
> Of tongue-deprivèd Philomel,
> What time the lone star looms his light
> Upon the purple brow of night,
> And fragrant pine-odours embark
> Within the deep-endonjon'd dark—
> Like memories.

But it was here, alas, that I committed my great crime—almost as bad as that of the Ancient Mariner. Nightingales are delightful in the evenings, but if they perpetrate their lyrics (as I have done in this book) at inconvenient times, just outside one's bedroom all night long, they are fatiguing. Next night, both my wife and Steele wondered what had become of them. I will not describe the deed ; but one who throws stones at nightingales on the flanks of Helicon can never hope to become a poet ! After we had found numbers of infected people and pools at Moulki, the neighbouring village, we examined Thebes—where there was much less malaria on the Rock. Then we visited the oracle of Trophonius and the springs of Lethe and Mnemosyne at Livadia, where many Greek doctors called upon me—intelligent men, some of whom spoke English ; and, lastly, I investigated Bœotian Orchomenos, once a city of rich traders but now a miserable village in which almost every child had an enormous spleen due to malaria.

The process of examining children's spleens in villages is always amusing. When you first arrive they run away shrieking, the dogs bark, the fowls cackle, and irate mothers stand at their doors. Then one of your attendants catches one of the children and brings it forcibly to you, and you impress a penny into its dirty little palm, let it go, and smoke a cigarette.

Presently all the children stand round you in a ring with finger in mouth. After yawning you pat one on its head, insert your fingers under its left ribs (where the spleen is), give it a penny, and let it go. Presently you know the proportion of children with enlarged spleen in the whole village—a useful measure of the amount of malaria in a place, which we investigated here, in Mauritius, and in Cyprus, and which is now (and was previously) generally employed. By the same artifices we can usually persuade the infants to allow us to take a droplet of blood from their dirty little fingers—another useful test. Soon one is beloved by the whole village—priest, headman, innkeeper, mothers, children, dogs, fowls, and fleas.

On 4 June we returned to Athens. I worshipped at the Parthenon every day—but after four pleasant days said goodbye to Mr. Steele and our friends, and we left for Constantinople. There we spent two days seeing the sights; and we heard the "howling dervishes" performing their remarkable oratorio of rhythm-music to the words *Alla illa Alla* and the reading of the Koran. Then for home by *Oriental Express* through Sophia and Belgrade, and we were back on 15 June 1906.

During this visit I had formed a theory which was of considerable interest to scholars. It is certain that so recently as 1866 malaria entered Mauritius, the idyllic island of Paul and Virginia, for the first time and completely altered life there, driving the planters from their villas round the coast into the central highlands. To my medical apprehension Greece in the time of the Persian Wars could not possibly have been in its present condition; while now severe malaria haunts almost every valley and the course of almost every stream, except in a few areas like the Attic Plain, leaving only the barren hills, where there is little water, safely habitable. How could we imagine, for example, that wealthy Orchomenos could have been as malarious in those days as it is now—when, moreover, there was no quinine. I argued therefore that the disease must have entered Greece about 500 B.C., or later, by the introduction of *Anopheles maculipennis* or of infected soldiers or slaves from Asia; must have then crept slowly up the valleys and destroyed their rural prosperity, as it did in Mauritius; and so must have played a considerable part in the subsequent decadence of the country. The argument is very strong by itself, but on my return home I proposed to have the subject investigated historically; and at the same time tried to raise a fund in England to help the Greek Antimalaria League. Fortune favoured us. William Osler had now been appointed

Regius Professor of Medicine at Oxford. He invited me to lecture on "Malaria in Greece" before the Oxford Medical Society, which I did on 9 November 1906 [83]. We raised £500 very quickly; and, what was even better, found a most enthusiastic historical investigator—Mr. W. H. S. Jones, afterwards Fellow of St. Catherine's, Cambridge. Assisted by Mr. A. E. Shipley and others, he examined much ancient literature, and his book, *Malaria, a neglected Factor in the History of Greece and Rome*, with an introduction by myself, appeared in 1907 (Macmillan & Bowes); and his *Malaria and Greek History* in 1909 [89]. Both confirm my conjectures. Savas came to stay with us at Liverpool in September 1906, and Sir Alfred Jones gave him a lunch on the 17th. The Greek League did some good work with quinine-distribution, and cleared malaria from the banks of the Ilissos at Athens by "training" that small but famous stream [91]. I visited Greece again in 1913 and 1917 (pages 513, 519).

Mr. D. Steele was a remarkable man, who went to Copais from a Scottish farm, consolidated the company's property there, and even conducted a small war against the "hillmen" in defence of it. He had a high opinion of the Greek peasantry. Personally I was disappointed in the state of the country. If the Greeks had used their climate, scenery, and historical associations as the Swiss have used their mountains they would have been the more prosperous for it; but the trains were few, the roads bad, the inns wretched and, instead of nursing its true interests, the nation seemed to abandon itself to a debauch of politicalism. But I was filled with enthusiasm, and in my lecture at Oxford figured Greece as Andromeda about to be saved from the devouring monster of the waters by the League as Perseus; and, in accordance with my "Heliconian Philosophy," I exhorted the Greeks to return to the worship of Athene. Here is the peroration of my address—somewhat exalted, perhaps, but sincere. Talking of the Parthenon, I said:

"And who was the god for whom that temple was built—which of all those gods, who are not dead as some imagine, but who live now and will live for ever, because they are the everlasting types of our own spirit? That goddess, whose birth and victory were recorded on the pediments of the Parthenon; who sprang, not from the common zygosis of Nature, but full-armed from the head of Zeus at the touch of Fire and

Toil ? . . . The Parthenon was the temple of Science. The great figure of Science, standing before it, dominated the whole of Greece. At its gates, even, stood the figure of Hygeia, the Science of Health, whom we now invoke. Science is the goddess whom we serve, as did the ancient Athenians, because we know that she and she alone can save us from these elements of the Deep which oppress us. We are her servants. We honour not the baser gods—the quack remedies, the sham philanthropies, the false knowledges, the mock philosophies, the whining pities, the lying politics which keep men down in the depths. We acknowledge only the intellect which sees the truth and smites the evil. Let us pray Pallas Athene to revisit the land where she was born."

Our last days in wonderful Greece were saddened by a telegram announcing my mother's death on 31 May 1906. We could not arrive in time for the funeral.

In August 1906 Jones, who had long had business relations with the Congo, took Boyce, Dr. J. L. Todd (one of the most distinguished workers in our expeditions) and myself to Brussels to pay our respects to King Leopold II of Belgium—a large subscriber to our School. The King and his suite received us (standing) on the 31st in one of the private apartments in the palace, and spoke to us jointly and severally for an hour on sanitary matters. One of the conversations was so amusing that I recorded it immediately afterwards. The King, Jones, and I were standing together and talking about the Canary Islands, where Jones had large interests. Jones, who was short, energetic, and direct, exclaimed: "We should work up those islands: your Majesty should go with me there." The King, who was immensely tall, thin, and regal (he spoke English perfectly) looked down at Jones, laughing, and said: "What! *I* go with *you*, Sir Alfred Jones!" The latter was not a bit abashed, and when the conversation veered to sleeping sickness, cried: "*We* have done a great deal in this line: your Majesty should follow us." This really nettled the monarch, who exclaimed haughtily: "It is for me to lead and you to follow, Sir Alfred Jones!" Jones never turned a hair; and presently we went into the most wonderful lunch I have ever eaten—or rather tasted. There were sixteen courses; but I sat on the King's left, and could scarcely do justice to them while he talked to me; for if I laid down my knife and fork for a moment the footman behind my chair instantly snatched

the dish away! After lunch, Boyce, Todd, and I were presented with the insignia of Chevalier de l'Ordre de Leopold II. The King struck me as being an extremely able sovereign, who recognised that his people's welfare was bound up with his own.

A few weeks later my wife and I attended the Quatercentenary Celebration of Aberdeen University, Dr. and Mrs. Levack being our kind hosts. I had a terrifying experience. Numbers of honorary degrees were conferred on 26 September 1906, and before the ceremony I was ushered into a room which I thought was set apart for the LL.D.'s, of whom I was to be one. But I fortunately noticed in time that the other inmates of the room were much more grave and dignified than Doctors of Law usually are, and so just managed to escape being made a Doctor of Divinity by mistake! King Edward VII and Queen Alexandra opened the new buildings before all of us next day, and Lord Strathcona gave a banquet to 5,000 guests.

In September 1907 my wife and I went to Berlin, where I read a paper at the Congress of Hygiene on "The Prevention of Malaria in British Possessions" [84].

Now at last in 1907, ten years after I had found *Plasmodia* in mosquitoes, I was asked by a British colony, Mauritius, to advise it regarding its malaria; but even now I was invited only to advise, not to manage the work, and the invitation emanated originally from the French planters and officials of the island. It reached me through the Colonial Office (letter dated 9 May 1907). My expenses were guaranteed, and I was even asked my professional fee: I replied, £1,000, if the colony could afford it, otherwise nothing; and the fee was allowed. Sir Alfred Keogh, Director-General of Medical Services at the War Office, appointed Major C. E. P. Fowler, R.A.M.C., to assist me, especially regarding the troops in Mauritius; and I left England on 23 October and, travelling by a Messageries-Maritime steamer from Marseilles, reached the Seychelles in November—lovely, mountainous wooded islands, peopled by 24,000 negroes who, Governor (Sir) W. E. Davidson told me, were stalwart and loyal Britons with a death-rate of only 16 per 1,000 (there was no malaria). Steaming thence down the east coast of hilly Madagascar, we touched at Tamatave, and then at Réunion, the French island, a magnificent snow-capped volcano, and so reached Mauritius on 23 October 1907.

This "pearl of the Indian Ocean" is situated just north of the southern tropic, 550 miles east of Madagascar, contains

705 square miles, and is shaped like an inverted saucer, with a high plateau (reaching 2,711 ft.) in the middle, surrounded by a rim of jagged hills sloping down to the sea. Its rich cane-fields support a considerable French population, including many wealthy planters (with highly aristocratic names derived from *émigrés* at the Revolution); but the former negro slaves had now been supplanted by about 260,000 Indians, who had been introduced since 1835 as "indentured labour." The Governor and some of the officials are British, the others French; the planters own many beautiful villas; the whole place was humming with industry and prosperity; numerous trains carry the business men daily from the capital, Port Louis, on the coast, to the loftier and cooler towns of Quatre Borne, Vacoas, and Curepipe in the centre; and a British battalion is stationed at Vacoas. Of course the scenery and climate are beautiful, as one would expect from Saint-Pierre's classic story of Paul and Virginia, which I had read in a prize-book obtained by my brother Claye at Spring Hill School in 1874, now possessed by me. But in 1866 this paradise was overshadowed by the sudden entry of malaria, hitherto unknown in Mauritius. The disease is said to have killed a quarter of the population of Port Louis in 1867, and then certainly spread slowly all round the coast and even to smaller areas in the highlands. The planters forsook their sea-side villas, the labourers were decimated by sickness at harvest time, the children became wizened with enormous spleens, and prosperity declined. In 1906 the death-rate reached 40 per 1,000, two and a half times that of the fever-free Seychelles.

On arrival at Port Louis I was whirled off to Government House (Le Réduit), where Fowler was awaiting me and where we were delightfully entertained by Sir Cavendish Boyle, K.C.M.G., and his niece, Miss Lane. Then, taking a pleasant little house at Vacoas (oh the mosquitoes!), we set to work with Dr. Lorans, Director of the Medical and Health Department (kind, good man, who died a little later), and spent a strenuous three months visiting place after place, taking the spleen-rates of the children (page 495), examining mosquitoes, and organising preventive measures. Sir Cavendish at once lent us the services of an accomplished entomologist and active helper, M. d'Emmerez de Charmoy, and gave us 6,000 rupees, with which we employed ten "moustiquiers" and thirty labourers. I cannot possibly detail our doings here—they occupy 186 pages in my *Report on the Prevention of Malaria in Mauritius* [87]. On 24 January 1908 I delivered a two-

PLATE X.

MAURITIUS 1908.

hours' lecture to the Société Médicale of Mauritius—in the middle of a raging thunderstorm; on 29 January a lecture before the Governor at Curepipe; and on 8 February another (I believe) to the Mayor (Dr. Laurent) and the Councillors of Port Louis. The planters entertained us everywhere, and we lunched with the Hon. M. Leclézio, his wife, and their *eighteen* sons and daughters, all together. On 20 January we gave a picnic tea to the Governor, Miss Lane, and most of the notable ladies and men of Mauritius in the middle of the deadly Phœnix Marsh near Vacoas!—and we all afterwards distributed charity to some scores of the sick Indian children who lived there—the marsh was drained a little later and the malaria vanished. A battalion of British troops had arrived here just before we did. They had all been provided with excellent War Office mosquito bed-nets; but these had not been hung, because the local authorities disputed as to who should provide the hooks; and the result was that there were seventy-one cases of malaria, five deaths, and many invalidings, costing thousands of pounds! I judged that malaria had been brought to Mauritius and Réunion in 1866 (simultaneously) by the introduction of *A. costalis*, the Sierra Leone death-bearer, the common *Anopheles*, *A. mauritianus*, being innocent. My final scheme for malaria-reduction was estimated to cost 135,000 rupees annually—about £9,000—or 0·36 rupees per head of population and 1·2% of revenue. At last the sad time came for us, weary but happy, to say farewell to our friends (whose names will be found in my Report). The French newspapers complimented and caricatured me; the doctors gave us a great lunch; the Société Médicale made me its president; and we embarked on the s.s. *Melbourne* (which had brought me) on 25 February. But we were put into quarantine for a whole week in a square dock at Point de Galets, Réunion—intense heat, mosquitoes, and thirty French babies on board all crying night and day! Then, just as we were to leave, a cyclone commenced. We reached London on 28 March 1908. There I bought a four-seated Minerva car, which finally brought me to Liverpool, still alive.

Now for some winding-up notes. The "vacuum-theorists" (page 489) were still active; and I therefore attempted in my Mauritius Report [87] a mathematical analysis of the various factors which must influence the time-to-time variation of malaria in a locality. The difference-equation showing the effect of each factor was obtained; and, though I could not then solve it (the mathematicians did not help me), it was

easy to work out illuminating examples. A fuller analysis is given in the second edition [91], and I solved the general difference-equation still later [105] (*Science Progress*, vol. xiii, page 288, October 1918).

All this time the Liverpool School of Tropical Medicine had been doing good work. In 1902 Mr. William Johnston (page 468) had given the University a new block of laboratories in commemoration of the death of his daughter, Professor Rubert Boyce's wife, who had died shortly after the birth of her child; and one of the laboratories was given to the School. In 1904 the University inaugurated a Diploma in Tropical Medicine; and in 1905–6 we began to publish the *Annals of Tropical Medicine and Parasitology* in continuation of our *Memoirs*, of which twenty-one had already appeared. In addition to the teaching of students the School sent many of the best workers on expeditions to Africa and elsewhere, who certainly rendered invaluable but ill-requited service to the Empire. All this belongs to the history of the School and cannot be detailed here. As I could not have a seat on the School Committee, I was not responsible for all its doings and in fact did not approve of some of them—especially of a later tendency to obtain *réclame* by over-hasty methods. I must say this because many people thought that I was the director of the institution—which I was not—and blamed me accordingly. Sir Alfred Jones and the business men of Liverpool who subscribed to and managed the School did not always understand the niceties of professional etiquette in such matters; and I remember one of them once saying to Milne and me, "You are not advertising enough. Go on! go on! *Fool the public!*" I really believe that some of them thought we were purveyors of quack medicines. I fear that Boyce was never quite the same man after the death of his wife, and became possessed of a restless spirit which drove him to deal with matters regarding which he had little experience. Thus he rushed off for a few days to Ismailia in 1904, and to West Africa in 1905; and then wrote an inaccurate report regarding the latter, which merely confirmed the local authorities in their attitude of indifference to anti-malaria work (*Memoirs* XII and XIV of the Liverpool School). The climax was reached early in August 1895 when, an epidemic of yellow fever having broken out at New Orleans, Jones telegraphed personally to the mayor offering the city the services of *his* two experts on yellow fever, Professors Ross and Boyce—neither of us having ever seen a case of that disease! This telegram appeared immediately in most British papers. The

mayor thanked Jones and said he would welcome us if " without expense to service." Then Jones invited me by telegram to go; but, of course, I refused to do so, except under proper conditions. Boyce went, however; and after his return was appointed, apparently as an expert on tropical diseases, to one or more committees of the Colonial Office which I had never been asked to join. Next year he had a " stroke " which paralysed one side of him; but he recovered and stumped about with the aid of a stick, as full of fire as ever, but a pathetic figure. Owing, I think, to the kind offices of Jones, he was knighted during his recovery. Subsequently he paid several visits to the West Indies and wrote two books, *Mosquito or Man* (1909), and *Health Progress and Administration in the West Indies* (1910)—both on lines already quite familiar to previous workers. Gradually he became rather obsessed with the idea that most of the fever in West Africa is not malaria at all, but yellow fever. He even thought that all the children there have it and pass it on to the Europeans, as they do malaria, and, not possessing the calculative capacity, he could not understand that the epidemiology of malaria cannot possibly apply to yellow fever, though both diseases are mosquito-borne. But the Colonial Office easily melted before his Hibernian enthusiasm and sent him to Africa with a posse of doctors to confirm his speculation—of course he failed. He was very angry with me because I did not agree with him, but his health was such that no one could discuss the matter with him. Then he had another sudden stroke and died on 16 June 1911, while I was absent in London.

His friend, Sir Alfred Louis Jones, had preceded him. So far as I could see from the balance-sheets, Jones had subscribed very little to the School for a number of years past, and had not even paid all his share of the endowment of the chair held by me; but he was profuse in his hospitality on all occasions —when colonial governors visited Liverpool or our expeditions started; and he certainly kept us at the front, besides pushing many other schemes. He was suddenly taken ill in December 1909 and died on the 13th at Oaklands, Liverpool. A memorial column by Sir G. J. Frampton was erected by public subscription to his memory in the city in July 1913—whereon it is stated that " as Founder of the Liverpool School of Tropical Medicine, he made Science tributary to Civilisation in Western Africa and the Colonies of the British Empire." To Boyce's memory a mural tablet was set up at the entrance to his laboratory with the words: " The University that he helped to

establish and the School of Tropical Medicine which under his leadership has increased health and lessened suffering in the remotest regions of the world are his true and lasting monument."

Except for isolated efforts by Colonel W. G. King and some other officers, little actual malaria-reduction work had been done in India since I had left in 1899. In 1902 some work of this kind was started at Mian Mir, the military cantonment attached to Lahore—an absolutely flat area with an impervious subsoil, which is covered with pools during the rains. This enterprise was inadequate and was adversely criticised by me and others [77, 90]. Nothing further seems to have happened except that Dr. Leslie, who had been secretary to Surgeon-General Harvey in 1898, and who I thought always opposed everything I suggested,[1] was made for some inconceivable reason Sanitary Commissioner with the Government of India —perhaps the most important medical post in the country; while all the "vacuum-theorists" and devotees of the great god Non Possumus clamoured against me. I was much concerned, and, happening to get an introduction to the Secretary of State for India (I will not say when), spent an hour alone with him in his office pleading my cause on behalf of the million people who are said to die of malaria in India and of the millions more, mostly children, who suffer from it. He sat before me like an ox, with divergent eyes, answering and asking nothing —and ended by doing as little. He was the personification of the British nation in the presence of a new idea; and as I left I could almost fancy seeing the prophetic handwriting on the wall over his head, *Mene, mene, tekel, upharsin*. In 1908 I was invited to attend a great medical congress to be held in Bombay 22–25 February 1909—my return fares to be provided; and, hoping that something might come of it, I made the long journey. With several other delegates, I was most hospitably entertained by the Governor of Bombay, Sir George Sydenham Clarke (now Lord Sydenham) and was delighted to find no mosquitoes in his house. But when I read my paper at the Congress on 22 February I was attacked by all the devotees mentioned in a united body. Subsequently I heard from several others at the Congress, who were indignant at this treatment, that the whole matter had been arranged beforehand and that I had been sent for in order to be publicly baited. That is the last I have ever seen of India. In October of the same year

[1] I have recently been told (June 1922) by a distinguished man of science in India, who knew Leslie well, that this was the case.

an Imperial Malaria Conference was held at Simla, where many resolutions were passed, but, I think, nothing essential done. A commission was, however, appointed to report on the alleged experiment which was still being conducted at Mian Mir, with a view to finding whether mosquitoes can really be reduced—whether two and two really make four. Their report appeared next year and was destructively criticised by me and Colonel King in *The Lancet*, 5 November [90] and 3 December respectively. It appeared that " since 1905 the princely sum of £66 a year was allowed for the work. This, be it remembered, for a difficult country of 8 square miles occupied by 5 regiments and a total population of 16,000 people ! The sum amounts to 1 anna (about 1*d*.) per head of population." At the same time, I estimated that the disease was costing government four hundred times that amount for the military in the station alone ; and in my book I suggested that the whole " experiment " was a public hoax. The Report of the Commission was itself simply incompetent. Early in 1911 Lord Crewe, who was now Secretary of State for India, visited my laboratory in Liverpool and agreed to receive memoranda from King and myself on the whole subject of malaria prevention in India ; these, dated respectively 4 and 3 April 1911 were duly despatched and at least moderated the opposition. They were not published. What has happened in India since then I have not attempted to follow.

After returning from Mauritius in 1908 I spent most of my time writing my book *The Prevention of Malaria* [91]. It was issued, with contributions by twenty men who had made special local studies, including Gorgas, Howard, Celli, Savas, E. Sergent, A. Balfour, O. Cruz, and M. Watson, by Mr. Murray in 1910. The second edition, with an additional mathematical section on the " Theory of Happenings," appeared next year.

On returning from Greece in 1906 I had the idea of collecting and publishing my *Fables* (page 48) and giving the proceeds to my fund for the Grecian Anti-malaria League. With the capable assistance of Dr. John Sampson, Librarian of Liverpool University, the book [113] was beautifully printed and bound at my expense, and was brought out early in 1907 and sold by Henry Young & Sons of South Castle Street, Liverpool, at the price of half a crown. A most flattering review of it by no less an authority than Sir Edward Russell (afterwards Lord Russell), editor of *The Liverpool Daily Post and Mercury*, appeared in that journal of 5 January 1907, and this was followed by an equally pleasant review, which was itself a

work of art, by that distinguished critic, the late Mr. Dixon Scott, in *The Liverpool Courier* 11 January 1907. There were also notices in *The Lancet* and *The Spectator* of about that date, but the *Manchester Guardian* thought that the speech of the Bear in "The Piteous Ewe" (page 96) was not too sagacious! I think I got ten pounds for my fund from this book. Many of my colleagues at the University, especially Professor Strong (Latin), were pleased with it.

Similarly, while writing my *Prevention of Malaria*, I thought that it would help the Cause if *In Exile* (pages 97 and 493) were to appear at the same time. In 1909 I had read *Multitude and Solitude*, by John Masefield. It gives a remarkably accurate picture of the tropical disease Sleeping Sickness, the only mistake being that my friend Sir David Bruce's great work is attributed to me! I wrote to point this out to the author, and at the same time sent him a copy of *In Exile* [112]. In return, among other nice things, he said (I am allowed to quote it): "We read *In Exile* continually, with increasing admiration. It is the only poem I ever read which kept me awake half through the night. It is a wonderful work. I cannot think what the publishers were thinking of. It is a great loss to the world that it is not more accessible." Of course this, coming from such a poet as Masefield, decided me—and Dr. Weir Mitchell, the American physician, poet, and novelist, had already reviewed the poem [112] favourably in the journal of the *American Medical Association* for 7 September 1907. So I put up *In Exile* with a number of my old verses which were more or less connected with the malaria work, wrote a preface, and asked Mr. Murray to bring it out under the title of *Philosophies* [114] simultaneously with the *Prevention of Malaria*. Mr. and Mrs. Masefield helped me with the proofs and usefully analysed each piece (I was rusty for literary work); and both books appeared in September 1910. There were several very warm reviews, especially by Sir Edward Russell (*Liverpool Daily Post*, 3 October 1901), and by that fine poet, Mr. Lascelles Abercrombie (*Liverpool Courier*, 19 October 1910). Later Sir Herbert Warren mentioned my work in a brilliant article on "Lucretius" (*The Observer*, 11 February 1917), and Mr. Cloudesley Brereton sent me the following pride-compelling epigram:

> Some verse in fragile clay is wrought
> And some in marble. You anneal
> The puissance of your glowing thought
> In flawless lines of tempered steel:

and he added regarding the quatrains of *In Exile*, amongst other pleasant remarks, that " In France . . . they would have been at once accorded national recognition, *as a new note in the national lyre*." On the other hand, *The Hospital* of 15 October 1910 savagely told the author " to return to his own sphere of scientific research "—something like what Keats had been told to do by a similar critic ! But I have been quite content with my audience, fit though few ; and the book reached a second edition in November 1910, and a third one in June 1911.

I suppose that *In Exile* is almost unique in one respect, that it was written during a long and tedious medical research ; but, as explained in the preface, the omission of many unfinished stanzas gave it a cast much more sombre than is natural to me. In fact it thus became, without intention of mine, a unified epic of that rare but gloomy Spirit of Discovery which wanders forth alone into the wilderness—where only, if we survive, may new continents be found.

At the end of 1910 I went again to beautiful Stockholm to receive the honorary degree of M.D. from the Caroline Institute, with M. Emmanuel Nobel—it had never before been conferred on a foreigner. I arrived, via Gottenburg, on Friday 9 December, saw Count Mörner and all my old friends, was at the Nobel prize-giving and banquet next day, dined with the King on the 12th, received my degree next day (accompanied with a gold ring and a high hat with a gold buckle in front), lunched and dined with Sir Cecil and Lady Spring Rice at the Embassy, visited Salzjobaden with Fru Edgren, dined with M. Nobel, and (heigho !) for home on 16 December. Even the helot of science has his compensations.

In 1909 Sir William MacGregor had been transferred from the Governorship of Newfoundland to that of Queensland. On 21 April 1911 he wrote to me indignantly that he had spoken very plainly to a certain office on my behalf and also about the absurd African yellow-fever speculation—in which, like me, he did not believe. In June 1911, just before poor Boyce's death, I received the intimation that H.M. King George V intended to include my name in the Coronation Honours for promotion in my Order, and I was given the accolade at the investiture on Thursday 6 July.[1] On such occasions the letters and telegrams of congratulation are delightful ; and we received 296 of them—from students, friends, colleagues,

[1] I was the fourth out of the seven who I think received it largely in consequence of 20 August 1897.

co-workers, Lord and Lady Derby, the Lord Mayor, Mr. Winston Churchill, Colonel Seely, shop-keepers, hotel-keepers, servants, and even vacuum-theorists. But the inevitable tragedy followed—or rather did not follow. I had arranged to take Mr. Masefield and Dr. Carnegie Brown for a short holiday to Capel Curig in my motor-car. Masefield wished me to join him in writing a great drama there; but I was too busy answering all the congratulations. Thus the world lost a masterpiece indeed!

I must now mention a holiday amusement of mine which interested several friends, especially Sir William Osler. I had long studied phonetics in connection with the subject of euphony of verse, but found the spelling employed in that science to be if possible uglier than that used by spelling-reformers or by the public. So I set myself the following pretty problem—what is the most beautiful way of rendering English correctly by means of the existing alphabet *only*? A number of factors, often overlooked by reformers, must be considered; and the test is English verse—which becomes ridiculous at once with a bad scheme. After examining I think every scheme which has been or may be suggested, I concluded that there is no perfect one possible, but that there are half a dozen good ones. Unfortunately these are so equal in value that it is impossible to judge which is the best! But in 1911 Messrs. C. Tinling of Liverpool put ten of my poems set in one of these schemes into beautiful type [115], under the name *Lyra Modulata* (fifty copies without preface or explanation); and I sent copies in lieu of Christmas cards to some of my amazed friends. Several asked me what language it was, and one suggested Esperanto. Since then I have preferred another scheme, which I call Musaic Spelling; and I still hope to horrify the world some day with a second edition of *Lyra Modulata* (see also *Science Progress*, vol. viii, 1913–14, page 367).

Another holiday task was the following. In 1903 my brother, now Major-General Charles Ross, C.B., D.S.O., published a book called *Representative Government and War* (Hutchinson, London), which was one of the first books to warn us of the German danger. Of course it was angrily denounced by many newspapers; but I believe it helped Lord Roberts and the National Service League in their appeal for an adequate army—which, if the politicians had listened to it, would probably have prevented the Great War. I joined the League, and thought to help it by expressing the same doctrine in the form of a satire called *The Setting Sun*. This was to be anonymous,

because I hoped that the public would attribute it to some politician at least, and so read it! Unfortunately when it appeared (Murray, 1912), the authorship leaked out, and the world, learning that it was written only by a professor, took no further interest in it whatever. Nevertheless, the satire has a novel argument and a well-considered doggerel style, and would have gone far a hundred years ago—nowadays the din of the political newspapers makes such a work inaudible. But there is some large truth behind the banter and the book will be dug up again some day.

My next literary venture was my *Indian Shepherds* (pages 102, 290), which had been commenced in 1898 as a sequel to *In Exile* but which has remained a short fragment. I wished to have above all that "surge of rhythm" mentioned on page 30, and for this purpose had long experimented with various measures in order to ascertain which is the best one for a translation of the *Iliad*—using phonetic spelling and correct rhythmical elocution, not the vile "prosification" of rhythm now so frequently employed in recitations. Blank verse consists of six dissyllabic crochet-feet to the line or bar, with only an occasional trisyllabic foot, and is therefore too slow. Finally I chose the same six-foot measure, but with a much larger proportion of trisyllabic feet of all possible kinds, and put the Shepherds into it, with a fitting alliterative phone-music. I expected some lively criticism of it when it appeared; but no one took any notice whatever. It was preceded by some verses by Thomas Hardy on a sordid subject—which reminded me of a goat I had seen tied to the columns of the Propylæa!

A very pleasant affair happened early in January 1912. Before that, some members of the Russian Duma had paid an official visit to our Parliament, and now a return visit to be made by a representative British party was organised, largely by Professor (Sir) Bernard Pares of the School of Russian Studies in our University, and I was asked to be a member of the party. Lord Weardale was to be our chief (as the Speaker could not go), and there were five of our bishops, with Lords Charles Beresford, Cheylesmore, Ampthill, Peel, Hugh Cecil, and General Bethune, Colonel Yate, M.P., Sir Valentine Chirol, Sir D. Mackenzie Wallace, Sir Albert Spicer, Sir W. Mather, and other Members of Parliament and gentlemen interested in Russia—twenty-seven altogether. We left Victoria on Monday morning 22 January and, after crossing the Russian frontier from Germany, travelled in the Tsar's own train to Petersburg, which we reached on the morning of the 25th. A number of

the young Russian nobility were appointed to look after each of us severally, to wake us at 8 a.m. and to *try* to buckle on our sword-belts. There were two banquets a day, each commencing with the zakuska, a gourmet's cold collation with caviare, vodka, and a cigarette, after which the doors were opened for the serious eating!—one banquet cost 100 roubles (£10) a head. Before and between these feasts, special parties of us paid special visits and received addresses; Viscount Peel and I had to do with the learned societies, and I received an illuminated album from the Russian Malaria Society. Theatres, ballets, and balls kept us going till 3 a.m. On the morning of 29 January the Tsar and Tsaritza talked to us for two hours at Tsarkoe Selo. We were ranged in a semi circle, and the Tsaritza came first and spoke to each of us in turn in perfect English, and then the Tsar did the same. She was a beautiful lady, but, as I said at the time:

> There was a listening fear in her regard;

as if she had divined the awful future. He wore naval uniform (I think), was goodness and cheerfulness itself, and told us how like he was to our own honoured sovereign. Then they showed us all their children. . . . That scene, I am sure, lives in the mind of everyone present—and I suppose we have all witnessed with the actuality of a nightmare the horror that followed. Do not think that the gods and the prophets are dead; behind them stand the laws of nature; and the millions of Russia now expiate that crime according to those laws.

After the audience we saw the wonders of the palace and lunched there; and on the following day went on to Moscow, where there was a similar round awaiting us. We left for home on 5 February in the Tsar's train—surprised, not only that we lived after all these banquets, but that we never felt so well. The temperature was many degrees below freezing, and there were awful snow-storms; but in the well-built houses and trains one never even felt the cold and could sleep with one blanket and a coverlet—as in Sweden; yet the Russians do not have catarrhs nearly as frequently as we do. I visited several magnificent hospitals and, during a halt on the return journey, was driven some miles in a sledge to see a Pasteur institution for rabies in a village. We were present at a meeting of the Duma, visited the holy city near Moscow, and saw "Hamlet" acted in Russian *à la* Craig.[1]

[1] Most of the party left London wearing fur coats and Astrakhan hats, but I was too busy to make such preparations, and joined them in an appalling leather-lined fishing ulster surmounted by a "top-hat." These were not admired by

After the death of Sir Alfred Jones in 1909 Mr. W. H. Lever (now Lord Leverhulme) kindly became the chairman of our School. Possessed of immense business capacity, he is also a Heliconian, as proved by his beautiful houses and pictures; and has, moreover, shown the world how to solve the housing problem by the example of Port Sunlight—men make houses, but also houses make men! He added £800 a year to our School income straightaway.

Now, in 1912, we had spent thirteen pleasant years in Liverpool and had many friends there. My salary was small, but the ample university vacations allowed us numerous holidays for fishing or sea-bathing—at St. Mary's Loch and Aberfoyle in Scotland, in Guernsey and the Isle of Man, at Llandulas, Corwen, Capel Curig, and Lake Ogwen in Wales, and at Loch Arrow and Roundstone in Ireland. In 1903 we moved from 26 Devonshire Road to 1 Aigburth Vale, and in 1910 to 1 Princes Park Terrace, overlooking that beautiful park. In June 1910 my former salary of £600 a year was increased to £800 a year (in both cases without share of students' fees). About the same time a number of careful "enumerative studies" on malaria and trypanosomes were done in Liverpool by those excellent workers Drs. David and John Thomson and Dr. G. C. E. Simpson, and others, under my direction [93]; and Sir Edwin Durning-Lawrence gave us £1,000 for experiments on the effect of extreme cold on such affections. I followed Sir Patrick Manson as President of the Society of Tropical Medicine (London) in 1909, and was Vice-President and Member of Council of the Royal Society in 1911–13 (I had received one of its Royal Medals in 1909). Jones left a large fortune at his death, and some of it was given to the School by his executor. I made an effort to have it employed for founding a Bureau of Tropical Medicine, distinct from teaching; but, as usual, the money went in bricks and mortar—for building new premises for the School. None of it was (at least in my time) given to increase the salaries of the workers. The Colonial Office established the Bureau, in part, under the capable direction of Dr. A. G. Bagshawe. In 1908–10, Colonel J. E. B. Seely, one of our Members of Parliament for Liverpool, was Under-

my companions; but the tables were turned when we reached Russia. There the crowds mistook the respectably dressed members for Russians and maintained dead silence—until I appeared amongst them, when they recognised a true Briton and cheered themselves hoarse! Thereafter the order always was, "Ross, wave your top hat, will you!" It was, in fact, admitted that my top hat made the alliance between Britain and Russia, and so helped largely to win the war!

Secretary for the Colonies and insisted on my being made (at last) a member of several Colonial-office Committees (West Africa, Bureau, Research Fund). After Boyce's death in 1911 that office appointed a committee to consider his theory of yellow fever in West Africa, and made me a member—considerably against my will because I was sure that that theory was unsound. Much later it was forced to despatch General Gorgas to West Africa to decide the point, and before his death in London on 4 July 1920 he sent his assistant, General Noble, to take my opinion, and, I understand, rejected the theory. I suppose that the money spent in this way would have sufficed to give effect to my projects of 1901–2 in all the principal towns of West Africa.

Ever since 1899 I had been anxious to take consulting practice in London (page 466). I could have done so immediately after receiving the Nobel Prize in 1902, but it was impossible for me to leave Liverpool just after they had given me an endowed professorship there at the same time. In 1912 another opportunity, which I was obliged to take or refuse forthwith, occurred ; and I had many calls to London. The School was unwilling to raise my salary above £800 a year ; and the pension which I should receive from the University (based upon my own subscriptions) would be little more than £100 a year at the age of 65. I therefore resigned my professorship at the end of 1912, bought the lease of Sir Francis Laking's house in London, 18 Cavendish Square, and put up my door plate in the orthodox fashion—being at the same time appointed Physician for Tropical Diseases at King's College Hospital. But Sir William Lever (he and Osler had been made baronets in 1911 when I was knighted) kindly arranged for me to retain connection with Liverpool as Professor of Tropical Sanitation at a salary of £400 a year, my duty being to go there from London every term to deliver my course of lectures on malaria and sanitation. This post was not pensionable, so that the small pension which I had partly earned by nearly fourteen years' service in Liverpool was lost, my contributions being returned to me. Moreover, there was the condition that I should not teach or give any course of lectures on Tropical Medicine outside Liverpool—which, I was told afterwards, was a most unusual and indeed improper restriction on the part of any educational body to make, because it prevented me from imparting knowledge to others. The appointment was formally made for five years ; but both Lord Leverhulme (who resigned the chairmanship of the School shortly

afterwards) and I thought that it was intended to be for life partly in lieu of pension. When the five years expired, however, the people who now manage the institution ended the appointment, and at the same time refused to give any pension either to me or to my friend, Mr. A. H. Milne, C.M.G., who had been our capable secretary for eighteen years and had done much more to make the School than all of the others just referred to put together. Even Lord Leverhulme tried in vain. Thus Boyce's fine promises of 1899 (page 370) were never redeemed. Needless to say, that in spite of losing £800 a year I have never regretted leaving the Liverpool School of Tropical Medicine and going to London—though, of course, the war prevented the full development of my programme.

Before starting work in Cavendish Square I went on my last real malaria expedition—Spain, Cyprus, Greece. Leaving London on Sunday, 2 March 1913, I travelled by train from Paris through far-famed but barren Castille, reaching Madrid next day at 11 p.m. There I saw a few of the sights, and remember that I suddenly met Colonel Seely, M.P., then Secretary of State for War, standing in front of the great Velasquez in the Prado-Bacchus and Mars teaching the peasantry how to drink (Heliconian) wine, a picture which is Shakespeare and Cervantes in colour. Leaving Madrid on the 5th, I arrived by train and carriage at the New Centenillo Silver and Lead Mines at Linares in the Sierra Morena, the owners of which in London had asked me to advise their capable medical staff regarding cases of Mediterranean Fever which had occurred in the place. There I found a delightful little British colony, with bungalows, tennis-courts, and hospitality complete, living much respected among the Spaniards of those ancient brown hills. Thence on (?) 12 March through Cordoba (where, owing to a miscalculation, I had no time to see anything) and Andalusia down the rocky cañon to Algeciras and Gibraltar, where I took steamer to Alexandria, and from there by a small steamer to Famagusta in Cyprus (20 March). I went there by request of the High Commissioner, Major Sir Hamilton Goold-Adams (through the Colonial Office), to advise regarding malaria, as I had done in Mauritius (my fee was £200 plus expenses). The moving spirit was Dr. R. A. Cleveland, the Chief Medical Officer. Dr. Patrick, one of the medical officers, was appointed to help me, and I was given the assistance of Mr. Mehmed Aziz, a young Mohammedan who had been educated in the States and spoke perfect American. After I left he was made Chief Sanitary Inspector of the island, and the fine

results which followed were largely due to him, acting under the orders of Dr. Cleveland. I have no space to describe this delightful visit—snow-capped Troödos (? Olympus), the barren Messaorian plain, the sprouting wheat-fields, the blossomed almonds, the bonny Turkish children humming their lessons in the schools (page 495), the mediæval ruins of Famagusta, the eternal Kytherean spring leaping down from the heights of Pentadactylon, the lofty castle of St. Hilarion, the ruined temple and foaming shores of Paphos where Aphrodite grew from the wind-blown froth, and the far-off snowy summits of Asia, white against perfect skies. There I did again the old work and retold the old story, and on 17 April left my many new-found friends for Egypt, and thence to Greece.

Athens once more, and the mountained marsh of Copais (25 April). Savas and Steele met me (page 494); and, with Sir Francis Elliott, the British Ambassador, and Dr. Savas, I had a long interview with M. Venezelos, the Prime Minister of Greece, on malaria prevention—a gentleman, not like a politician, who listened to all we said, but I do not think did much. Steele and I spent the Greek Easter Sunday on Helicon, visiting the ruins of the temple of the Muses, fast by the brook Hippocrene, which sprang from the hoof-stroke of Pegassus on the overlooking summit. Alas, now, the temple is nothing but a pavement, with the poor pillars lying prostrate here and there, while huge tortoises, merry asses, and obscene swine pollute the spot. There and thereafter the following words, which sum up one side of my "Heliconianism," grew in my mind:

THE SIGNPOST

Adventurous Stranger who dost dare to climb
Huge Helicon: Remember and mark well
What every several Muse may deign to tell,
If thou would'st hear Their symphony some time.
Not who hymn Heaven always roll the rhyme—
Who scan th' unutterable Stars, foretell;
But haply, as each far-firèd pinnacle
Fumeth at sunrise, sing Their song sublime.

But if thou be too proud thou shalt be thrown
To dismal valleys where foul fog distills,
And the thick tortoise clambers to the stone,
The root-uprouting hog his belly fills,
And asses bray their wisdom to th' eternal hills.

I was home on 3 May 1913—via the Corinthian Canal and Brindisi. Athens had given me its honorary M.D. degree in 1912.

Mr. Murray had already asked me to edit *Science Progress*, a quarterly which had appeared in 1906 and contained chiefly original articles. My first number was that of July 1913, and I gradually introduced many new features, especially regular contributions by experts on recent advances in particular branches of science. The magazine survived the war and doubled its sales, and Miss Edith Yates has been its secretary and mine since I was connected with it.

When the war broke out we were all on holiday (in that beautiful summer) at Bourne End, on the Thames. My elder son, Ronald Campbell, had just obtained his commission as Second Lieutenant in the Royal Scots Regiment. He had been educated at Sherborne, was a born soldier, popular with all his comrades, and somewhat like my grandfather, Lieut.-Colonel Hugh Ross. Of course the Royal Scots were flung into the rearguard in the retreat from Mons. Near the end of August 1914 he was reported missing; and some months later, after torturing suspense, I heard from Germany, from his brother subaltern in "D" Company, Lieut. C. G. Graves (Sir Edward Grey's nephew), that he had been killed and the survivors of the company taken prisoner close to the village of Audencourt, near Beaumont, on 26 August.

On 22 June 1915 my friend, Mr. John Holt, the Liverpool shipowner, died. He was one of our most genuine supporters, gave a medal to the School, and even protected his own ships against the entry of mosquitoes. He was an enthusiast for West Africa and a great reader of my *Philosophies*, which he always kept near him during his illness. There was an excellent obituary of him by Mr. E. D. Morel in *The African Mail*, 2 July 1915. My friend, Dr. Walter Steeves, of Liverpool and Cavendish Square, died on 11 December 1915, just after my return from Egypt and Salonika; and his death was followed by that of my Swedish friends, Professor Count K. A. H. Mörner, of Stockholm, on 30 March 1917, and Professor Seved Ribbing, of Lund, on 14 February 1921.

For years I had been toiling at the attempt to fix mathematics on the general theory of epidemics, and in 1918 the Royal Society published my first paper [96] and gave me the capable assistance of Miss Hilda P. Hudson. After a second paper, the war interrupted our studies; but so little interest was taken in them by the "health authorities," that I have thought it useless to continue them since then.

In May 1912 I had been asked by the Board of Education to report on the finances of the London School of Tropical

Medicine, as the Board proposed to give it a grant—which it did on my recommendation. My enquiry showed that many of the workers there were receiving very poor salaries, without pension ; and in *The British Medical Journal*, 7 February 1914, I called general attention to the way in which these and other medical investigators were being exploited by the British public and Government.[1] In the meantime, in order to rouse some sense of justice in the matter, I had formed the rash scheme of following the precedent of Edward Jenner, the discoverer of vaccination, who, early in the nineteenth century, had petitioned Parliament for compensation for professional losses caused by his scientific work and had justly received £30,000 in consequence. I did the same for my much humbler work, but, I confess, with some satiric laughter and full expectation of failure ; and forwarded my petition on 8 November 1913, after consulting lawyers, to Mr. Lloyd George, then Chancellor of the Exchequer. Of course Cleon, who squanders half a million a year on politicians, refused it (*Science Progress*, vol. x, page 315, 1915). After the war I submitted it again to the new Chancellor of the Exchequer, Mr. Austen Chamberlain, son of the statesman who had so skilfully established schools of tropical medicine at other people's expense, who also refused it. Meantime the British Science Guild and the British Medical Association, which supported me, formed a conjoint committee to report on the general question of state compensation for unremunerative medical discoveries (*Science Progress*, vol. xiv, page 635, 1920) ; and, led by our revered and eloquent chief, Sir Clifford Allbutt, we went on deputation to the Lord President of the Council, Mr. A. J. Balfour, on 2 March 1920, but received no definite reply (*Science Progress*, vol. xv, pages 113 and 285, 1920, and *British Medical Journal*, 6 March 1920). He did not seem to have troubled to read our Report (drafted by me) ; but, on 20 July 1921, stated in reply to a question in Parliament that he was not in favour of the course we had suggested, because " The difficulty of apportioning merit for even the greatest of discoveries is often overwhelming ; monetary rewards would lead to jealousy instead of co-operation among research workers." Of course the same arguments might be urged against the giving of any honours or rewards whatsoever, from Victoria Crosses down to Peerages (*Science Progress*, vol. xvi, page 286, 1921) ; and if men of science are not to be paid, why should wealthy

[1] I had previously argued the case that every country should give adequate rewards for unremunerative discoveries (*Br. Med. Journ.* 8 December, 1906).

politicians receive large salaries and pensions for public work which they should be honoured to do for nothing? After this failure we approached the Royal Commission on Awards to Inventors; but it refused to consider medical discovery and invention, because (it argued) doctors had always been noble enough to do such public work for nothing! We may be sure that the lawyers on the said Commission do not follow their example.

For the reasons mentioned on page 426, and exhaustively discussed in our Report, these decisions were most unwise. They amount to this, that while the nation is to continue paying very large sums annually on subsidies for current "researches" which may, or, more probably, may not prove useful, it is to pay nothing at all for any discovery, however important, already achieved by private persons without expense to the State; that is, the nation buys eggs, though addled, but not chickens when hatched! But the case is worse than this. Notoriously, many well-to-do persons make a habit, when sick, of "sponging" on the altruism of medical practitioners; so, it seems, the wealthy British Empire is to continue sponging for ever on the altruism of medical investigators—both cases of what is called "chousing the doctor." But the English are not a "bright crowd," and are easily misled by their politicians.

In 1916 the British Science Guild appointed a committee, with me as chairman, to consider the employment of scientific experts by Government offices and municipalities; and decided that such employment, without proper payment, in addition to mere expenses, was unjust in principle and open to abuses in practice. It therefore asked a number of public bodies to accept this view. Assent was given by all of them except the Colonial Office and the London County Council. I was at the time a member of four committees of the former, two of which were, very improperly, unpaid. I asked it to reconsider its decision; it refused, and I therefore felt myself obliged to resign all its committees. But I did so with considerable regret.

On Friday, 23 March 1917, many of my poems, including selections from *In Exile*, were read by Miss Miriam Lewes, Mr. William Stack, and myself at Lord Leverhulme's beautiful house on Hampstead Hill; Sir Herbert Warren presided, and Sir Rider Haggard and a large audience were present. In November 1919 Mr. Murray produced my little book called *Psychologies* [121], containing four of my "drametta" written

years ago, namely, "Otho," "The Triumph," "Evil," "The Marsh" (pages 96), and "The Boy's Dream," containing some lyrics taken from *Edgar* (page 45)—the first two had already appeared in *The Nation*. On 17 May 1920 he published *The Revels of Orsera* [122], the romance written by me in 1894–5 from my drama, *The Deformed Transformed*. A fierce critic said of Sir Harry Johnston and myself that we ought to be ashamed of ourselves for trying to win literary fame after having acquired some of that article in other fields; it did not occur to the poor man that, with me at least, literature was the first love, and that my romance had been written a quarter of a century previously. When Peter Paul Rubens was Ambassador in England, an English courtier found him seated at his easel. "So His Excellency the Ambassador plays at being a painter," exclaimed the courtier. "No," replied Rubens, "His Excellency the painter plays at being an Ambassador." Science is the Differential Calculus of the mind, Art the Integral Calculus; they may be beautiful when apart, but are greatest only when combined. No one can be blamed for trying to combine them.

On 8 February 1916 my daughter Dorothy was married to Lieut.-Colonel James William Langstaff, D.S.O., R.A.M.C.; and on 18 September 1917 my daughter Sylvia to Captain James Blumer, then of the Durham Light Infantry, now of Darlington. Both marriages were held at St. George's Church, Hanover Square; and at the latter one several pieces of music, composed by me forty years previously, when I was a student, were beautifully played by the organist. My son, Charles Claye, went to Magdalen College, Oxford, in May 1921.

In July 1908 I had entered the Territorial Force as Major in its Medical Service, and in November 1913 was promoted to Lieut.-Colonel in the same. From 21 December 1914 I was appointed, with that rank, Consulting Physician in Tropical Diseases to the hospitals for Indian troops in England; and in July 1915 was sent to Alexandria, with Captain D. Thomson as my staff officer, for similar work in connection with the terrible outbreak of dysentery in the Dardanelles; but, when this ceased, was allowed to relinquish the post, after a short visit to Salonika, and to return to England (30 November 1915). In 1916 severe malaria appeared among the troops in Salonika, and our great chief, General Sir Alfred Keogh, started special hospitals and researches in all the eight British Commands to deal with the invalids, and appointed me Consultant in Malaria to the War Office (where I was given a room later),

Moustiques et Malaria
Dessiné aux Etats-Unis par le Docteur Jaccoud
Tu peux, Tu peux te réveiller mon Vieux!
Si Major, réveille-moi!

(From a Mauritian Newspaper, 1908.)

On His Majesty's Service.

RR (C.S.I.) has much pleasure in accepting the kind invitation of RR
(DUPLICIS EQUESTRI DIGNITATE DECORATUS
VIR TORPEDINI INVULNERABILIS:
M & M EXPERTUS: MATHEMATICUS
MIRABILIS: PISCATOR MAGISTER:
MUSICUS HARMONICUS: CONVIVATOR
GENIALIS: MILES INTREPIDUS:
BOTULINI AMATOR: DOCTOR
OMNIS DAMNATI: SOCIUS SOCIETATIS
REGALI: ETC.: ETC.: ETC.)
for the gathering at the Savoy
on 10th October 1918.
DORA
NOTICE
OWING TO OUR ARTIST BEING ABSENT ON WAR SERVICE ACCURATE PORTRAITS AND DRAWING ARE NOT GUARANTEED BY THE FIRM
Local Government Board, Whitehall.
(16)

CARICATURES OF R. ROSS.

from 15 February 1917. On 26 November 1917 I visited Salonika again on behalf of the War Office, and returned on 18 January 1918. I was promoted full Colonel on 5 February 1918, and remained at the War Office till I was demobilised on 17 September 1919, when I was appointed Honorary Consultant in Malaria to the Ministry of Pensions.

Here are two caricatures of me: one from a Mauritian newspaper in 1908; and the other done by my friend, Colonel Richard J. Reece, of the Ministry of Health, on a large official envelope for a dinner which I gave to my War Office friends.

My health was never the same after I went to Alexandria in 1915, and remained poor, sometimes seriously so, up to 1920, when I remembered, rather late, the injunction: "Physician heal thyself," and, with the help of Dr. Parkes Weber, Dr. A. Macbeth Elliott, and Dr. A. S. Woodwark, at once recovered some years of age! But my time at the War Office was nothing but pleasant to me, especially with such chiefs or colleagues as Sir Alfred Keogh, Sir John Goodwin, Sir William Horrocks, Sir William Leishman, Sir Edward Worthington, Colonels A. L. A. Webb, J. R. McMunn, W. C. Smales, S. Fleming, and Majors Challis and Angus Macdonald. It was the first time that I had been employed on executive work connected with my own subject; but it came too late. I had long urged many more (real) researches on malaria, especially regarding treatment, but almost in vain; now the taxpayer is justly punished by having to pour out colossal sums on this account. Really we still remain barbarians in all such matters—very far indeed from the Heliconian ideal!

Since the war we have been engaged in treating thousands of pensioners for malaria; and Sir A. L. A. Webb established, on my advice, a number of special clinics for the purpose, ably managed by Drs. G. Basil Price, R. E. Drake-Brockman, W. Broughton-Alcock, E. Marshall, T. H. Jamieson, and others. I think that we have had some success [98].

I saw nothing of the fighting (except on the London front!), but the following experience may interest the reader for a concluding episode of this long tale. Before proceeding to Salonika in 1917, I was ordered to stop *en route* at Taranto, the southern Italian port, where a number of our men were becoming infected with malaria. I left England on 26 November with Colonel J. C. Robertson, who was in sanitary charge of Taranto, and my staff-officer, Captain F. W. O'Connor, Temp. R.A.M.C., and we decided upon an anti-mosquito campaign, which proved very successful next year. On

13 December Colonel Plunket (?), King's Messenger, Captain O'Connor, and I embarked at Taranto on the old French cruiser *Château Rénault*, which was now used as a transport for French troops to Greece. We were accompanied by another transport and two destroyers for escort, all French. It was a lovely evening and night, and early next morning (bright and calm) we were in sight of the mountainous Greek coast at the entrance of the common trade channel between St. Maura (the Leucadian Rock) on one side and Cephalonia and Ithaca on the other—I had been through it thrice already. I shared an upper cabin with Colonel Plunket, and had my coffee at 8 a.m., while we were zigzagging into the entrance of the channel (where there was a Greek fishing boat), and was preparing to have my bath, when there was a tremendous crash which nearly flung us down. When I went on deck there was some confusion, but our ship, though motionless, was certainly not sinking fast; so I went below again to look for my watch, purse, and gold charm,[1] which were in my hand when we were struck. I could not find them anywhere in the cabin after long search, and therefore put on an ordinary cork float over my Gieve waistcoat and went on deck again. We were in a land-locked bay close to the Leucadian Rock (where Sappho is supposed to have drowned herself); the destroyers were alongside and the French soldiers were getting into them; but as our ship seemed to be all right I went down again to the cabin, exasperated at losing my uncle's gold watch! I never thought of a second torpedo! Watch not to be found; so I put on my heavy military overcoat, with Sir J. G. Frazer's *Studies in Greek Scenery, Legend and History*, and Baedeker's *Greece* in its pockets, and went on deck for the third time with my Government despatch-case and my valise. Almost everyone had got into the destroyers and Colonel Plunket and O'Connor were not to be seen; so I scrambled across a pontoon into the destroyer *Mameluke*, getting one leg wet, while a sailor threw my valise and despatch-case across after me. Both destroyers then sheered off. Meantime several British "drifters" had emerged from somewhere and were popping away at something; one came between us and the *Château Rénault*

[1] This was a little golden pig taken from an Aztec's tomb, supposed to bring wonderful luck and given to me by a Panama banker for advising about his child in 1904. We *tested* it scientifically at Taranto by placing it before various card-players in the club—who always lost heavily! When I got back to Taranto the men there said it was really a devil which had caused the torpedoing of our ship and had then, fortunately for me, vanished with my watch and purse! I suppose that I really put them all down together among my bed clothes.

as we sheered off. Then came another, and another, terrific explosion; and (? a few) minutes later up went the stern of our old ship and down she went (with all my bedding, confound it, and gold watch). We were so closely packed on the *Mameluke* that we could not move or even sit down. Now came our turn: *we hunted the submarine in that land-locked bay.* The French soldiers round me said there were three of them; there was only one. The two destroyers followed each other at great speed, round and round in a circle, dropping depth-charges. They looked like greyhounds, and the foam at their bows looked like gnashing white teeth. The depth-charges in our wake sent up columns of water fifty feet high. The bay was now alive with drifters popping away at something, and the other transport was dodging about among the rocks close to the shore, to escape torpedoes. The rising sun was like Apollo in the east, and the rippled sea was blue as heaven. We were all wild with delight. I said to myself: "Ulysses and Penelope must be seated on Ithaca there, watching the scene." Presently, to our amazement, up came two great-sea-planes, circling over us. A poor old French soldier with grey hair just in front of me began to whimper because he thought that they were going to drop bombs on our little packed boat; but all the rest of us were cheering ourselves hoarse like the audience at a cinema, and such is humanity, that I remember saying to myself: "*I have never enjoyed myself so much before.*" But, in a moment, we enjoyed ourselves still more. Suddenly all the soldiers began pointing in one direction, and one behind me said, "Voyez, monsieur." There, two hundred yards from us, was the deck of an emerging submarine. She had been touched by one of the depth-charges. Her crew were jumping off her deck into the sea, one after the other, as fast as they could, like frogs. In another minute a storm of shells and shot ploughed up the water round her. Then our captain yelled out "Asseyez vous." We were going to fire off our big gun. We were packed so close that we could not sit down; and when our gun did go off we nearly went over. Our shell took effect; up rose the stern of the submarine, and then, slowly, down she slid, as her victim had done, leaving a number of pink heads dotting the water—Boches clamouring to be saved. A Frenchman near me was handing round pistols to shoot at them, but our captain promptly stopped that. Boats went out and rescued eighteen of the German crew; they came aboard naked and shivering, but happy! For some reason we were all happy together. The aeroplanes circled

over us, the observers waving handkerchiefs. We cheered, they cheered, the drifters cheered. Presently (about 11 a.m.) we were collected in a bay. Not a man had been lost, except a few of the poor engineers of our ship, who had been killed by the torpedo. Then we all raced off together. A French naval officer invited me to *déjeuner* in the cabin—about 12 ft. square. The other officers were eating ; and there in the corner sat the Boche captain of the submarine, clothed and fed, but with two Frenchmen standing over him with pistols—a determined but melancholy little man. We had an uproarious breakfast. After it I remembered a flask of rum in my Gieve waistcoat, and poured out a glass for each of the French officers and myself. One glass remained—but I hated, and we all hated, the Boche captain. Well, suddenly, somehow, I passed him the glass. He sprang up, saluted, clicked his heels together, and drank it off at a gulp. So we landed him at Patras, and then, tearing up the Gulf of Corinth, reached our destination, Itéa, near Delphi, only two hours late the same evening.[1]

This was on Friday, 14 December 1917 ; and Major Pullar, commanding the British camp at Itéa, succoured us. On the 16th we ascended to the Delphian Oracle. There, as of yore, were the Great Gorge and the Castalian Cleft ; but now the temple was nothing but a pavement, the treasuries were empty, the gods had become mountain eagles, and the voice of the pythoness was the voice of the wind. We asked the oracle what was the cause of all these human troubles, and Apollo replied (in the wind !) :

I taught this lesson into minds of worth
That man himself should be the god of earth,
Even like Me, omniscient and bright,
In perfect wisdom, perfect beauty, dight.

But men would not be taught
And climbing higher fell—
A fancied heaven sought
But reach'd a real hell.
Now in their folly school'd
They've found what they desire—
By evil idols ruled,
To die in blood and mire.

So ends my story.

[1] My friend Mr. Arnold White kindly moved the British Admiralty to ask the French Admiralty for a copy of the official report of these occurrences. It is now in my possession. An account in verse by me (not official) was given to the Royal Institution in a lecture on Science and Poetry delivered on 4 June 1920.

Sir William MacGregor retired from the Governorship of Queensland in 1914, bought the property of Chapel-on-Leader in Berwickshire, and died there in July 1919. Alan Milne died on 21 January 1919. Our great Lord Lister died on 10 February 1912. Dr. Manson was made K.C.M.G. in 1903 and G.C.M.G. in 1912, and died on 9 April 1922. Our revered master, Laveran, died on 18 May 1922. On Wednesday, 3 July 1913, after my return from Solonika, the King made me a K.C.M.G., in addition to my K.C.B., with some very kind words; and he gave the same honour to General W. C. Gorgas, of the American Army shortly before that officer died at Millbank Hospital, in London, on 4 July 1920—happy in the experience that his countrymen had employed him for the work that he was fitted to do.

APPENDIX

1. HONOURS AND AWARDS

Parkes' Memorial Gold Medal, Netley, 20 March 1895. Fellow, R. Coll. of Surgeons, England, 11 April 1901. Fellow, R. Society, London, 7 June 1901. Silver Medal, Society of Arts, London, 15 June 1901. Companion of the Bath, 26 June 1902. Cameron Prize, Edinburgh University, 26 July 1902. Nobel Prize in Medicine, 10 Dec. 1902. Barclay Bronze Medal, Asiatic Soc. Bengal, 20 May 1903. Sc.D. Trinity Coll. Dublin, 20 Dec. 1904. Officier de l'Ordre de Leopold II, Belgium, 31 Aug. 1906. LL.D. Aberdeen, 26 Sep. 1906. D.Sc. Leeds, 19 July 1909. Roy. Gold Medal, R. Society, 13 Nov. 1909. M.D. Karolinska Instit. Stockholm, 13 Dec. 1910. Knight Commander of the Bath, 19 June 1911. Vice-President Royal Soc. 1911–13. M.D. Athens, 12 April 1912. Officier de l'Instruction Publique, France, 1 Nov. 1913. Bisset Hawkins Gold Medal, R. Coll. of Physicians, London, 19 Oct. 1914. Freeman, Soc. of Apothecaries, London, 9 March 1915. Knight Commander, St. Michael and St. George, 3 June 1918. Athenæum Club (Rule II), 11 April 1922.

II. HONORARY MEMBERSHIP OF SOCIETIES

Med. Soc. Nederland Ind., Batavia, 20 March 1901. Soc. de Méd. de Gand, 14 Jan. 1902. Soc. Medicorum Svecana, Jan. 1903. R. Physiographie Soc. Lund, 11 Feb. 1903. Manila Med. Soc. 1 March 1903. American Soc. Tropical Medicine, 19 June 1903. Soc. Méd. et Hyg. tropicales, Paris, 11 Dec. 1903. Lancashire and Cheshire Entomolog. Soc. 18 Jan. 1904. Acad. de Méd. Paris (Corr. étranger), 19 July 1904. Malaria Soc. of Greece, 12 July 1906. R. Accad. di Med., Torino (Corr. estero), 9 Nov. 1907. Soc. de Pathologie exotique, Paris, 22 Jan. 1908. Coll. of Physicians, Philadelphia, 5 Feb. 1908. Soc. de Med. Trop. Cuba, 25 April 1908. Hon. President, Med. Soc. Mauritius, 4 Dec. 1908. President, Soc. Trop. Med. London, 18 June 1909 to 1911. Russian Malaria Commission, 2 Feb. 1912. Imp. Military Acad., St. Petersburg, 2 Feb. 1912. Harveian Soc. of London, Oct. 1913. Inst. of Hygiene, London, 1 Dec. 1913. Soc. of Sciences, Upsala, 5 April 1919. Soc. Belge de Biologie, 6 Dec. 1919. École de Méd. trop. Bruxelles, 30 Dec. 1920. R. Soc. of Edinburgh, 4 July 1921. Acad. de Méd. Paris (Associé), 27 Dec. 1921. R. Accad. Med., Torino (Socio), 17 Feb. 1922. R. Soc. of Literature, London, 15 May 1922.

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INDEX

Abercrombie, Mr. L., 506
Aberdeen University, 499
Académie de Médecine, 357, 365, 366, 395, 525
Agra, 19, 258
Agramonte, Dr. A., 424, 425
Akola, 203, 212
Alcock, Surg.-Maj. A. W., 264, 265
Alexandria, 117, 518
Allbutt, Sir C., 375, 392
Almora, 10, 11, 18
Almquist, Dr. E., 456, 478, 479
Alsatia, S. S., 38–40
Amœbæ (intestinal), 196, 281, 283
Ampthill, Lord, 509
Amsterdam, 475
Andamans, 63, 64, 68–73, 476
Anderson's College, 488
Ankylostomiasis (hookworm disease), 280, 281, 284, 285, 320
Annett, Dr. H. E., 368, 376–388, 394, 416, 436, 442, 481
Anopheles (a genus of mosquitoes), 207, 208, 210, 211, 219–239, 246, 256, 258, 260, 261
  attitude of, 207, 221, 382, 408
  claviger (or *maculipennis*), 342–352, 416, 417, 496
  costalis, 333, 381–388, 394, 423
  culicifacies, 221, 230–232, 234, 237
  eggs of, 223, 233, 234, 328, 329, 344, 403
  fuliginosus, 260, 265–267
  funestus, 333, 381–388, 394, 423
  larvæ of, 234–237, 292, 321, 328, 329, 383–386
  listoni, 320, 322
  rossi, 292, 303, 310, 325, 326, 358
  stephensi, 222, 223, 231–234, 236
Anophelina (sub-family of Culicidæ), 219
Apothecaries, Society of, 37, 40, 525
Appia, Mr. K. N., 192
Appoo, 42, 63, 65, 101
Army Medical Service, 199, 200, 499, 519
Arrhenius, Prof. Svante, 482
Ascaris (round worms), 281, 283
Assam, 311, 319, 322, 323
Aston-Binns, F., 21, 22, 23, 25, 32, 83, 103, 104, 109, 110
Aston-Binns, Rev., Mrs., Miss, 21, 32, 83, 103–105, 110
Austen, Mr. E. E., 374, 376–388, 394, 442, 481

Bagshawe, Dr. A. G., 511
Balch, Dr. L., 492
Balfour, Dr. A., 490
Balfour, Rt. Hon. A. J., 357, 516
Bamber, C., 30
Bangalore, 44, 57, 59, 62, 89, 91, 93–100, 126, 178–199, 201, 232, 233
Barwell, Capt. and Mrs., 5, 20, 22
Barred-back Mosquitoes. See *Culex*
Bastianelli, Dr. C., 335, 348–352, 392, 398, 400–410, 480, 481
Beauperthuy, L. D., 123–125, 424
Beerlekom, Dr. B. van, 481
Behring, Prof. E. von, 477
Benares, 19
Benson, Dr., 95
Beresford, Lord C., 509
Berhampur, 102
Berkeley, Dr., 380
Berkeley, Dr. W. N., 440
Berlin, 499
Best, Dr., 448
Bestall, Rev., 89
Bethune, Gen. Sir E. C., 509
Bhawani River, 74, 93
Birds (used), 265
Bignami, Dr. A., 121, 127, 194–196, 207, 288, 335–354, 366, 392, 398, 400–410, 480, 481, 485
"Black Spores," 295, 299, 302, 304, 305, 308, 310, 311, 327, 361, 386
Blackwater Fever, 116
Blanchard, Prof. R., 169, 170, 171, 280, 306, 424
Bland-Sutton, Dr. (Sir J.), 248, 253, 411
Bliss, Hon. Sir H. W., 201, 202, 393
Blood corpuscles, 120
Bloxam, Miss R. B., 83
Bloxam, Mr. and Mrs. C., 103
Bolan Pass, 59–62

Bombay, 42, 59, 134, 185, 235, 240, 504
Boyce, Prof. Sir R. W., 354, 367–372, 377, 381, 391, 410, 424, 435, 437, 459, 468, 476, 477, 485, 486, 488, 498, 502–504 507, 512, 513
Boyle, Sir C., 500, 501
Brereton, Mr. C., 506
British Guiana. See Demarara
British Medical Association, 292, 293, 304–307, 312, 314, 325, 455, 458, 469, 516
British Medical Journal, 126, 147, 189, 196, 232, 233, 243, 246, 247, 248, 253, 254, 307, 332, 340, 344, 350, 366, 375, 377, 382, 394, 399, 402, 408, 411, 412, 417, 421, 440, 452, 455, 460, 485, 490, 515, 516
British Museum, 129, 374
British Science Guild, 516, 517
Broadbent, Sir W., 375, 419
Brock, Dr., 337, 345
Bromhead, Col., 68
Broughton-Alcock, Dr. W., 519
Brown, Dr. C., 508
Brown, Capt. E. H., 319
Browne, Mr. Gordon, 435
Brunton, Dr. (Sir) L., 392
Bruce, Sir D., 123, 133, 469, 484, 506
Buchanan, Dr. A., 517
Burma, 63–67, 86–91, 126

Cadell, Col. T., 69, 104
Cairo, 472, 490
Calcutta, 64, 258–278, 291–313, 318, 320, 325–360
Calandruccio, S., 122, 398, 399
Callender, Dr., 33
Cameron, Sir A., and Miss C. M. M., 5
Cameron, Sir C., 375
Cameron Prize, 469
Campagna, 340, 416, 417
Canalis, Dr. P., 121, 480
Canary Islands, 379
Cantlie, Sir J., 307, 392
Cardamatis, Dr. J. P., 495–496
Carey, Prof. F. S., 415
Caroline Institute, 477, 478, 479
Carrick, Rev., 23
Carrington, Miss A., 188
Caroll, Dr. J., 424, 425
Carter, Dr. W., 435
Carter, Dr. H. R., 425, 427, 492
Carter, Dr. V., 92, 126, 134, 480
Cecil, Lord H., 509
Celli, Dr. A., 118, 121, 122, 337, 391, 392, 394, 400, 405, 480, 481
Cercomonas (intestinal flagellates), 196, 281, 283
Ceylon, 78, 86
Challis, Maj., 519
Chamberlain, Rt. Hon. Austen, 367, 516
Chamberlain, Rt. Hon. Joseph, 284, 315–317, 366, 367, 376, 389, 394, 433–437, 439, 487, 488, 493
Chambers of Commerce, 389, 434–437
Chameleon, 26. 27
Charles, H.R.H. Prince, 479
Charles, Dr. T. E., 306, 333, 403, 407, 411, 440
Charles, letters to R. Ross, 335–353
Charmoy, M. d'Emmerez de, 500, 501
Chemistry, 7, 8, 32, 33, 75
Cheylesmore, Lord, 509
Child of Ocean, The, 20, 68, 70, 72, 73, 84–86, 93, 106, 476, 535
Chin-lushai Expedition, 86–89
Chirol, Sir V., 509
Cholera, 54, 64, 183–185, 214, 215
Christian, H.R.H. Princess, 493
Christian Victor, H.R.H. Prince, 494
Christophers, Dr. S. R., 322, 382, 423, 463, 481
Church, Sir W., 375
Churchill, Rt. Hon. W., 508
Christy, Mr. G., 475
Clarke, Sir G. S., 504
Cleghorn, Surg.-Gen., 188, 201, 203, 216, 243, 245, 254, 255, 259, 261, 281, 285, 390
Cleveland, Dr. R. A., 513
Coats, Mr. J., 437–440, 456, 460, 485
Coccidia, 169, 197, 211
Coccidia (a provisional name given to the malaria parasites in mosquitoes), 274, 275, 280, 297, 335, 423
Cole, Maj. and Mrs. R. A., 242, 246, 258
College of Physicians, 38, 82
Colonial Office, 347, 366, 315–317, 365, 374, 389, 391, 418, 434–437, 459, 474, 475, 485–488, 511, 512, 513
Cooke, Dr. N., 326, 428
Constantinople, 496
Coonoor, 94, 100, 101
Copais, Lake of, 494–497
Copenhagen, 478, 482
Cotterill, Misses, 21
Cottesloe, Lord, 6
Councilman, Prof., 491
Crawford, Col. D. G., 319
Cremonese, G., 399
Crescents, Crescent-Spheres. See *Plasmodia*
Crewe, Lord, 391, 505
Crichton-Browne, Sir J., 395
Crithidia (flagellate parasites of mosquitoes), 211, 237, 238, 258, 270
Crombie, Surg.-Col., 137, 152, 261, 263, 336
Crosthwaite, Sir C., 216, 245

Crown Prince of Sweden, 479, 480
Crudeli, T., 119, 397
Culex (a genus of mosquitoes), 128, 201, 210, 219–239, 260, 266, 383–386, 471, 472
fatigans, 156, 219, 232, 234–238, 257, 266–279, 291–313, 326
sylvestris (*Stegomyia scutellaris*), 209, 210
Culicidæ (family of mosquitoes), 219
Culicina (sub-family of Culicidæ), 219
Cunningham, Col. D. D., 126, 197, 234, 259, 260, 262, 263, 280, 285
Cyclones, 71
Cyprus, 439, 513

Dampeirou, Dr., 473
Daniels, Dr. C. W., 317, 321, 325–330, 358, 359, 382, 387, 421–423, 449–451, 481
Letters to R. Ross, 328, 450
Danilewsky, 121, 129, 263, 268, 396, 480
D'Arenberg, Prince A., 470, 471, 473
Darjeeling, 261, 278, 279, 286, 289
Dashwood, Mr. A., 21, 22
Davidson, Dr. A., 128, 306, 469
Davidson, Sir W. E., 499
Davis, Gen., 492
De Lotbinière, Col. A. C. Joly, 182
Deacon, Mrs. S., 444
Deformed Transformed, The, 68, 82, 90–93, 103, 535
Delphi, 522
Demerara, 158, 162, 163, 317, 421
Dennys, G., 30, 34, 35
Denton, Sir G., 441, 457
Derby, Lord, 488, 493, 508
Dionisi, A., 338, 343, 405, 480
Diptera (a class of insects including mosquitoes), 254
Dobson, Surg.-Col. A. F., 179–182, 191, 199, 329
Dodds-Price, Dr. D. F., 323
Doty, Dr. A. H., 440, 492
Drains, 182
Drake-Brockman, Col. R. E., 519
Ducommun, E., 477
Dudgeon, Mr. G. C., 289
Dumont, M., 470
Dunant, J. H., 477
Durham, Dr. H. E., 427
Durning-Lawrence, Sir E., 511
Dutton, Dr. J. E., 415, 441, 457, 481
Dysentery, 18, 196
Dyson, Dr., 326

Edgar, or The New Pygmalion, 45–47, 53
Edgren, Dr. and Fru., 482, 507
Edinburgh, 83, 469
Education, 20, 21, 23, 24, 43
Edward VII, H.M., 468, 483, 499
Elderton, A., 25, 34
Elderton, E., 21
Elderton, E. C., 5
Elderton, E. H. P., 5, 25, 34
Elderton, E. M., 5, 25
Elderton, H., 5
Elderton, J., 5
Elderton, M., 5
Elderton, M. C., 5, 10, 16
Elephantiasis, 127, 188, 200
Elliot, Dr. R. H., 243, 481
Elliott, Dr. A. M., 519
Elliott, Sir F., 494, 514
Elliott, Dr. J. H., 416
Epidemics, mathematical theory of. See Pathometry
Eugene, H.R.H. Prince, 479
Evans, Mr., 89
Ewart, Sir J., 306
Examinations, 24, 37, 83
"Eye-Forms" 165, 189, 261

Fables, 47, 48, 50–53, 72, 96, 505, 506
Fawcett, Major, 214, 215
Fearnside, Dr. C. F., 417, 455, 481
Federated Malay States, 357
Fedschenko, 122, 127, 153
Feletti, 399
Fermi, Dr. C., 399
Ficalbi, E., 129, 341, 343
Fielding-Ould, Dr. R., 381, 387, 388, 394, 395, 436
Filaria bancrofti, 122, 124, 127, 128, 135, 222, 229, 267, 321, 416
Filaria medinensis (Guinea Worm), 122, 127, 128
Filaria of birds, 260
Finlay, Dr. C., 124, 424, 427
Fischer, Prof. E., 477, 479, 480
Flagella, Flagellate Bodies (sperms). See *Plasmodia*
Flagellulæ (flagellate parasites). See *Crithidia*
Flagellates (intestinal), 196. See *Cercomonas*
Fleas, 168
Fleming, Col. S., 519
Fleming, Mr. and Mrs., 89
Forbes, Mr. A., 15, 16
Ford, Dr. R. H., 457
Fort St. George, 53
Foster, Sir M., 375, 421, 430
Fothergill, Mrs., 22
Foulkes, Dr., 217
Fowler, Maj. C. E. P., 499–501
Fraser, Prof. Sir T. R., 327, 409
Frazer, Sir J. G., 520
Fröhlich, Dr. A., 494
Furst, Prof. K., 478

Galli-Valerio, Prof., 457
Gambia, 439, 441, 457
Garrison, Col. F. H., 536
Garrison Surgeoncy, Bangalore, 57
Gauhati, 322, 323
George V, H.M., 507
Gerhardt, C., 119
Germinal Threads or Rods. See *Plasmodia*
Giles, Surg.-Maj. G. M., 259, 262, 280, 283, 319, 329, 330, 332, 358, 376, 394, 428
Glasgow, 83, 455, 456, 488, 489
Glazebrook, Sir T. R., 378
Gnats. See Mosquitoes
Gobat, C. A., 477
Godlee, Sir R., 375
Gold Coast, 388, 439, 444, 448, 449
Golgi, Prof. C., 121, 134, 194, 339, 396, 397, 480
Goodwin, Sir J., 519
Goold-Adams, Sir H., 513
Gorgas, Dr. W. C., 330, 433, 481, 488, 491–493, 512, 523, 536
Gorgas, Dr. W. C., letters to R. Ross, 453, 490
Gowers, Sir W., 419
Grand Canary, 441
Grassi, Prof. G. B., 122, 194, 263, 287, 288, 335–352, 366, 392, 398–412, 414, 440
Grassmann, 59, 95, 415
Greece, 133, 494–498, 514, 520–522
Gregarines. See *Stegomyia* (parasites of)
Grünbaum, Dr. A., 368
Guernsey, 511
Gustavus Adolphus, H.R.H. Prince, 479
Gwilt, Mr. J. D., 289
Gwynne, Mr. S. L., 106–109
Gymnosporidium, a name given to the *Plasmodia*, 279

Hæmamœba. Another name for *Plasmodium*, q.v.
Hæmogregarines, 255
Haffkine, Mr. W. M. W., 184, 243, 252, 253, 289, 327, 375, 484, 487
Haggard, Sir Rider, 85, 86, 517
Halford, Miss M., 5
Halford, J., 5
Halford, R., 6
Halford, Sir H., 6
Halteridium. See *Plasmodia* (of birds), 263, 287
Hamilton, Capt., 74
Hankin, Mr. E. B., 184, 258, 259
Harding, Capt., 214
Hart, Mr. E., 137, 138
Harvey, Surg.-Gen., 260, 269, 282, 285, 286, 292, 303, 310, 353–357, 390, 439, 440, 504
Havana, 330, 357, 378, 424–427, 433, 440, 465, 488
Haycock, Dr. H. E., 35, 36, 440
Hedin, Sir Sven, 482
Hefferman, Lieut., 214
Hehir, Surg.-Capt. (Sir) P., 142, 147
Helicon, 514
Herdman, Prof. (Sir) W. A., 377
Herodiasis, a name for multiple parasitic infections, 281, 283, 319
Hewett, J. F., 245, 247
Hindustani, 19, 43, 80
Hobday, Capt., 64
Hodgins, Capt., 441
Holmgren, Prof. E., 478
Holt, Mr. J., 439, 464, 515
Hong-Kong, 433, 440, 455
Horrocks, Sir W., 519
Hospital Assistants, 282, 284, 321
Howard, Prof. L. O., 481
Hudson, Miss H. P., 50, 515
Hughes, Surg.-Capt., 132, 133
Hugli, 20, 360
Hunt, Surg.-Col. S. B., 179

Italy, 79–82, 335–354, 396–413
Imago, adult mosquito. See Mosquitoes
Indian Lancet, 156
Indian Medical Gazette, 126, 191, 196, 278, 330, 354, 357, 402, 427, 428
Indian Medical Service, 29, 30, 37, 38, 40, 83, 199, 200, 356, 427, 468
Indian Mutiny, 11, 12, 18
Indian Shepherds, The, 45, 59, 102, 290, 509, 535
In Exile, 16, 21, 76, 77, 93, 97–99, 100, 185–187, 213, 218, 220, 225, 226, 493, 506, 507, 517, 535
Ingeborg, H.R.H. Princess, 479
Inkson, Surg.-Gen., 93
Inventors, R. Commission on Awards to, 517
Investigators, 184
Ireland, 25, 493, 511
Ismailia, 357, 378, 446, 470–474

Jackson, Capt., 68
James, S. P., 128, 399, 427, 481
Jamieson, Mr. G., 322
Jamieson, Dr. T. H., 519
Jancsó, N., 417
Jenner, Dr. E., 367, 516
Jenner Institute, 460, 466, 467, 470
Jessop, Mr., 68
Johnston, Surg.-Capt. C. A., 216
Johnston, Mr. W., 372, 468, 502
Joly, Prof. C. J., 415, 456, 493
Jones, Sir A. L., 367, 368, 372, 373,

375, 381, 388, 391, 435, 439, 459, 467, 469, 475, 477, 481, 488, 497, 498, 502, 503, 511
Jones, Mr. W. H. S., 117, 497
Journal of Hygiene, 212
Journal of Tropical Medicine, 307
Judgment of Tithonus, The, 45, 535

Kala-azar, 259, 281–284, 286, 317, 320, 321
Kanga, Dr., 216
Keary, Capt., 62
Keefer, Dr., 68
Kelly, Prof. H. A., 424
Kelsch and Kiener, 324, 480
Kennan, Dr. R. H., 123
Keogh, Sir A., 499, 518, 519
Kherwara, 241–257, 260, 285, 405
Kilborne, F. L., 123, 193
Kilkelly, Dr., 191
Kinchinjunga, 289
Kindersley, Mr., 204, 205
King, A. F. A., 124, 125, 288, 309, 485
King, Dr. Truby, 92
King, Col. W. G., 201, 212, 243, 428, 487, 504, 505
King-Harman, Sir C., 441, 449, 461, 465
Kingsley, Miss M., 379
Kipling, Mr. R., 66, 96
Kirkpatrick, Capt. W., 65, 67
Kissinger, R., 425
Kitasato, Prof., 217
Klebs, E., 119, 397
Klein, Prof. E. E., 82, 83, 86
Knox, Sir A. W. F., 102, 214
Koch, Geh. Prof. R., 64, 121, 123, 125, 126, 184, 183, 217, 313, 322, 333–335, 337, 339, 351, 366, 392, 400, 401, 408, 413, 421, 422, 430, 456, 467, 481, 485
Koch, Geh. Prof. R., letter to R. Ross, 408
Kruse, 268
Küchenmeister, 122
Kurseong, 278–280, 282, 284–289, 311, 320–325
Kyber Pass, 14, 15

Labbé, A., 263, 268, 276, 280
Labeo fishing, 75
Laboratory, 260, 262, 284
Laboratory Assistants, 262, 265
Lagos, 375, 388, 433–436, 439, 440, 444–448, 475
Laking, Sir F., 512
Lancet, The, 196, 243, 253, 307, 332, 340, 345, 353, 354, 376, 400, 402, 406, 411, 419, 421, 433, 487, 505, 506
Lancisi, 118, 123
Lane, Miss, 500, 501
Langstaff, Col. J. W., 518
Lankester, Sir E. R., 374–376, 388, 395, 536
Lanzi, 119, 397
Laurent, Dr., 501
Laveran, Dr. A., 40, 92, 101, 119–122, 125, 126, 128, 129, 194, 251, 271, 279, 287, 288, 293, 332, 334, 357, 365, 366, 388, 395, 397, 403, 409
Lavertine, Dr. J. C., 323
Lawrence, Mr. W. F., 435
Lawrie, Surg.-Col., 142, 178, 215, 354
Lazear, Dr. J. W., 424, 425
Lectures, 475, 489
Leclézio, Hon. Sir H., 501
Leishmania, parasites of men, 280, 319, 325
Leishman, Sir W. B., 519
Leopold II., H.M., 493, 499
Le Prince, Mr. J. A., 453, 492
Leslie, Surg.-Capt. J. T. W., 216, 255, 287, 318, 331, 355, 390, 504
Leuckart, 122, 169, 171, 280
Leverhulme, Lord, 469, 511, 513, 517
Lewes, Miss M., 517
Library, Medical, 262, 330
Lincei, R. Accad. di, 346, 401, 404, 406
Lindberg, Eric, 480
Lister, Lord, 35, 294, 303, 315, 316, 374, 375, 383, 412, 418, 455, 456, 460, 465, 467, 485, 523
Littlejohn, Dr., 469
Liverpool Courier, 506
Liverpool Daily Post, 505
Llandulas, 414
Longmore, Mr., 320, 322
Lorans, Dr., 500
Lorentz, Prof. H. A., 477, 480, 481
Lowder, G., 22, 23
Low, Dr. G. C., 128, 399, 417, 419
Lloyd George, Rt. Hon., 372, 516
Lund, 478, 482
Lund, Rev. T. W. M., 484
Lyra Modulata, 31, 88, 508, 535
Lyster, Dr. T. C., 425, 492

MacCallum, Dr. W. G., 178, 197, 253, 264, 266–268, 277, 288, 387, 400, 413, 480
Macdonald, Maj. A., 519
Mafadyen, Dr. A., 460
McGann, Surg.-Col., 95
MacGregor, Rt. Hon. Sir W., 375, 379, 433, 444–448, 455, 458, 460, 469–475, 481, 487–489, 507, 523
McKee, Surg.-Capt., 30, 37, 54, 56
McKendrick, Capt. A., 439, 449
Maclean, Surg.-Gen., 40
McMunn, Col. J. R., 519

Macnamara, Maj., 323
McNaught, Asst.-Surg., 319, 322, 323
McVittie, Surg.-Gen. C., 202, 212, 234, 240, 242–245
Madagascar, 499
Madras, 42–56, 73–78, 86, 91, 188
Madras Infantry; 10th, 44; 17th, 47; 4th, 58; Q.O.S.M., 59; 1st, 59, 62; 9th, 63–68, 73; 3rd, 74; 2nd, 86; 20th, 101; 19th, 101, 134, 138, 199, 214
Mahomed Bux, 262, 279, 284, 289, 302, 313, 322, 358, 360
Maiocco, Dr. F., 482
Major, Surg.-Col., N. B., 216, 234
Malaria, 14, 92, 101
  ancient knowledge of, 117, 118
  epidemiology of, 117, 118, 122–126, 130, 131, 161, 172, 176, 205–211, 218, 307–309, 313, 320, 322, 324, 384–386, 391, 407, 417, 422
  immunity in, 313, 323, 324, 337, 422
  importance and prevalence of, 115, 314
  nature and varieties of, 116, 117
  parasites of. See *Plasmodia*
  prevention of, 118, 320, 322, 331, 357, 360, 363–365, 369, 378, 386, 389, 415, 416, 418–421, 427–454, 457–459, 461–466, 470–475, 485–491, 494–496, 499–501, 513, 514,
  problem, the, 125, 126
  speculations regarding, 118, 119, 123–125, 128–131, 190–193, 212, 218, 286, 291, 293, 296, 299, 304, 307–309, 328, 339–343, 363–365, 383–386, 404–409
  spleen in, 116, 320, 325, 422, 495, 500
  treatment of, 118, 150, 151, 204, 313, 519
Malay States, 432, 440, 490
Malmsten, Dr. K., 482
Malta, 78, 135
Malta Fever, 132, 133
Man, Mr. E. H., 475
Manchester Guardian, 506
Mannaberg, Dr. J., 121, 127, 129, 456, 480, 494
Manson, Sir Patrick, 122, 124, 127–129, 131, 134, 135, 155, 156, 187–198, 207, 233, 234, 238, 245, 287, 288, 305–308, 318, 331–334, 339, 353, 366, 374–376, 388, 392–395, 403, 404, 410, 411, 413, 416–419, 435, 451, 483, 485, 490, 523
  letters to. See Ross, R.
  letters to Ross (begin), 152, 153, 163, 188, 192, 193, 247, 253, 315–317, 333, 334, 336, 353, 389, 390, 391, 401
  letters to Sir C. Crosthwaite, 216, 217
Manson, Sir Patrick, at Colonial Office, 247, 315–317, 366, 389, 391, 435
  his mosquito-theory, 128, 129, 131, 154–156, 163, 164, 193–197, 291, 293, 309
Manson, Dr. P. T., 417, 481
Marchiafava, Prof. E., 121, 122, 127, 142, 157, 194, 328, 400, 401, 481
Marchoux, Dr. E., 251
Marino, Dr., 122
Marshall, Col. E., 519
Masefield, Mr. John, 31, 506, 508
Mather, Rt. Hon. Sir W., 509
Mauritius, 379, 417, 496, 499–501
Maynard, Surg.-Maj. F. P., 261, 263, 278, 330, 390
Meckel, H., 119, 396
Medin, Prof. K. D., 478
Mehmed, Aziz, 513
Melanin. See *Plasmodia* (pigment in)
Mental ecdysis, 18
Metaxeny, 122, 123, 309
Metchnikoff, Dr., 251, 287, 332, 403
Mian Mir, 14, 504, 505
Miasma, 118, 119, 124, 126
Micron = one-thousandth of a millimetre = μ.
Microscope, diagnostic, 131, 137, 138, 153, 376
Microsporidia, a group of unicellular parasites, 280
Midges, 131
Milne, Mr. A. H., 368, 370, 371, 377, 381, 391, 459, 502, 513
Misanthropes, The, 48
Mitchell, Dr. Weir, 506
Mommsen, Prof. T., 477, 479, 480
Monsoon, 217, 220
Moore, Dr. (Sir) N., 29
Moran, J. P., 425
Morel, Mr. E. D., 515, 536
Mörner, Prof. Count K. A. B., 476–483, 507, 515
Morris, Dr. (Sir) M., 392
Moseley, Mr. C. H. H., 448
Mosquitoes (Culicidæ), 40, 42, 58, 89, 94, 118, 122–125, 127–131, 201, 202, 210, 219–239, 265, 326, 328, 329
  biting of, 135–137, 139, 256, 257
  books about, 129, 330, 484, 528, 530, 531
  breeding of, 58, 89. See *larvæ of*
  Brindled. See *Stegomyia*
  characters of some, 219
  Dappled-Winged. See *Anopheles*
  dissection of, 129, 135, 169, 170, 189, 224, 225, 255
  eggs of, 219, 223, 275
  Elephant. See *Panoplites*
  Grey, of Barred-back. See *Culex*

Mosquitoes, larvæ of, 89, 209, 219, 234, 235, 261, 313, 320, 328, 329, 331, 383, 384
length of life of, 128, 223, 229, 231, 271, 273
and Malaria. See Malaria and *Plasmodia*
migration of, 489
parasites of, 169–178, 187, 211, 237, 238, 258, 270, 280
poison glands of, 298–301
Mosquito Day, 223
Mosquito-nets, 94, 117, 136, 320, 331, 430, 431, 501
Mosquito-reduction, 58, 89, 320, 331, 357, 383, 384. See Malaria
Mosquito-theories, 122–125. See Manson, P.
Mott, Prof. (Sir) F. W., 375
Moulmein, 64–68
Municipal Commissioners, 179
Murray, C. H., 216
Murray, Mr. J., 506, 515, 517
Muspratt, Mr. (Sir) M., 439
Musser, Dr., 492
Mutiny, Indian, 11, 12, 18

Nagana, 123
Naini Tal, 18, 19
Nash, Mrs., 205, 209
Nathan, Maj. (Sir) M., 379, 386, 448, 457, 476, 477, 485–487
Natural History, 22, 23
Nation, The, 518
Nature, 377, 395
Navan, 25, 34
Naxalbari, 320
Neck, Dr. van, 376, 382
Netley Hospital, 40, 376
New Guinea, 421, 422
New York, 38, 440
Nicholls, Lieut., 68, 74
Nigeria, 416
Nilghiri Hills, 74, 93, 100, 191, 199–214
Nobel, A., 410, 476–483
Nobel, E., 479, 482, 507
Noble, Gen., 512
Nott, J. C., 123, 424
Northumberland, Duke of, 395, 419, 469
Notter, Col., 40
Nowgong, 319, 322, 323, 331
Nuttall, G. H. F., 123, 333–335, 376, 389, 395, 402–404, 406, 470, 481
letters to R. Ross, 334, 335

O'Connor, Capt. F. W., 519, 520
Oodeypur, 241, 258
Ootacamund, 191, 199, 202–214, 233
Operative Algebra, 58, 64, 493, 534
Opie, 178
Oscar II., H.M., 479
Osler, Dr. W. (Sir), 306, 491, 496, 508
Owen, Surg.-Maj. C. W., 146, 151, 158, 162, 245, 252, 253

Packenham, Maj., 214
Paget, Sir J., 33
Pakoko, 86–89
Pallaveram, 56
Paludism. See Malaria
Panama, 330, 357, 378, 490–493
Panoplites (Mosquitoes), 275, 325
Pares, Prof. Sir B., 509
Parkes Memorial Prize, 109, 129, 135, 151
Parlato-Hopkins, E. M. G., 80–82
Parona, E., 398
Passey, M. F., 477
Pasteur Institute, 332, 352
Pasteur, L., 121, 126
Pathometry, 504, 515
Patiala, 146, 151, 158, 162, 168, 201, 245
Patrick, Dr., 513
Pearson, Prof. K., 489
Parreau, Capt., 65–67
Pensions, Ministry of, 519
Penton, Maj. R. H., 473
Peshawur, 10, 12, 14, 15, 19, 31
Petterson, Prof. S. O., 482
Phagocytes, 138, 141, 159
Philosophies, 54, 71, 97, 506, 535
Physicians, Coll. of, Philadelphia, 525
Physicians, R. Coll. of, 6, 38, 82
Pickles, Dr., 448
Pigeon-shooting, 69
Pigmented cells. See *Plasmodia*
Pigment. See *Plasmodia*
Pioneer, The, 202
Piroplasma, 123, 193
Plague, 200, 284, 285, 288, 289, 291, 327, 341
Plasmodia (or *Hæmamœbæ*, the parasites of malaria)
asexual forms of, 120–122, 126, 127, 188, 215, 377
asexual forms, sporulation of, 120, 121
avian (birds' malaria), 121, 131, 198, 202, 238, 251, 254, 260–263, 270–313, 327, 377, 414
classification of, 121, 377, 387, 423
discovery of, 119–121
infection experiments, 119, 156–158, 165, 187, 190–192, 207, 211, 291, 296, 301–313, 336–341, 408, 416, 417
inoculation by blood, 119, 207, 418
inoculation by mosquitoes, 190–193, 207, 210–212, 301–311, 325, 336–341, 408, 416, 417

Plasmodia, nomenclature in, 335, 377, 423
pigment in (melanin), 119, 120, 224, 225, 228–230, 237, 248–250, 285, 324, 422
sexual forms (crescents, flagella, flagellated bodies), 120–122, 128, 129, 131, 135, 138, 187, 193–198, 218-239, 264
sexual forms, conjugation of, 197, 251, 253, 264, 266
sexual forms, development of in mosquitoes (zygotes, pigmented cells), 224, 225, 233–237, 267–276, 281, 345, 348–351, 377, 380–388
sexual forms, sporulation of in mosquitoes (germinal threads or rods, blasts, sporozoids), 271–275, 293–301, 309, 313, 377
staining of, 129, 162, 271, 273, 319, 326, 388
Plehn, Dr. F., 329, 480
Plimmer, H. G., 127
Plunket, Col., 520
Poet's Retirement, The, 50–53
Policlinico, 404–406, 411, 415, 440
Poona, 185
Portman, Mr., 68, 69
Port Said, 470, 471, 473
Poudrette, 185
Pressat, Dr. A., 471, 473, 474
Price, Dr. B., 519
Primrose, Sir J. U., 489
Proteosoma relicta. See *Plasmodia* (of birds), 263, 287
Protozoology, chair of, 494
Prudhomme, M. Sully, 477
Psorosperms (Pseudonavicellæ), 169–178, 295
Psychologies, 96, 517, 518, 535
Pullar, Maj., 522
Punkarbari, 279, 280–282, 284, 285, 289

Quarterly Journal of Microscopical Science, 395
Quetta, 59–61
Quinine, 118, 150, 151

Rajputana, 199, 212, 241
Random Migration, 439
Rao, Srinivasa, 196
Rasori, 119, 121
Ravenshaw, Maj. and Mrs. C. W., 241, 258
Raynaud, M. A., 473
Reece, Col. R. J., 519
Reed, Dr. W., 424–426
Reid, Lieut., 3, 4
Reid, Mr. W. J., 322
Reigate, 83
Relapsing Fever, 191
Residency Surgeon, Bangalore, 179, 180
Retzius, Prof., 482
Réunion, 499
Revels of Orsera, The, 103–105, 109, 246, 518, 535
Revue Scientifique, 395
Ribbing, Miss M., 478, 482
Ribbing, Dr. Seved, 376, 478–515
Rice, Mr. and Mrs., 95
Rice, Dr., 448
Rice, Sir C. Spring, 507
Richmond, Col., 68
Rivenburg, Dr. S. W., 326
Roberts, Lord, 13, 17, 62, 468, 508
Roberts, Rev. F., 414
Robertson, Col. D., 181, 212
Robertson, Col. J. C., 519
Roe, Surg.-Gen. W. C., 86
Rogers, Capt. (Sir) L., 277, 319, 320, 324, 330, 428
Romanowsky, 122, 129, 326, 388, 396, 400
Romanowsky method of staining, 129, 319, 326, 388, 480
Röntgen, Prof. W. H., 477
Ross, A., 5, 16
Ross, Gen. C., 16, 22, 26, 27, 82, 111, 376, 508
Ross, C. C., 457, 518
Ross, Sir C. C. G., 4, 9–16, 22, 27, 30, 31, 40, 82, 95
Ross, C. E., 5, 26, 33
Ross, Capt. C. R., 16, 19, 22, 26, 27, 41, 82, 110, 111
Ross, Miss D., 93, 518
Ross, Dr. E. H., 16, 474
Ross, Col. H., 3, 4, 515
Ross, H., 4
Ross, Dr. H. C., 16, 474
Ross J., 16, 82
Ross, Miss M. A., 16, 22, 27, 31, 40, 64
Ross, Lady M. C., 5, 16, 22, 27, 30, 31, 40, 82, 376
Ross, Sir R., 1–536
ancestry of, 3–6
birth of, 10, 18
children of, 93, 111, 457, 518
health of, 18, 20, 23, 27, 43, 75–77, 95, 96, 204–206, 312, 354, 519
honours of, 413, 440, 468, 507, 523, 525
fishing experiences of, 22, 25, 62, 68, 70, 71, 74, 75, 83, 89, 93, 94, 195, 246, 250, 511
joke (Scotch) of, 456
letters of, to Manson, 135, 139, 142, 147, 156, 157, 159, 160, 164, 165, 167, 169, 170, 173, 174, 187—198, 201, 202, 204, 207, 208, 211, 213,

220, 227, 231, 234, 235, 243, 246, 247, 248, 250, 253, 254, 256, 257, 259, 265, 267, 269, 272, 275, 277, 279, 280, 282, 284, 291, 292, 297, 300, 303, 305, 310, 311, 320, 323, 324, 333, 358, 359
letters of, to Laveran, 279, 287, 312, 313, 388
letters of, to Lord Lister, 294, 375, 383–388
leave of, 75, 77–86, 103–111
marriage of, 83
mathematical studies of, 23, 24, 49, 50, 54–56, 58, 59, 64, 67, 75, 83, 94, 95, 414, 415, 489, 493, 505, 515, 534
moulding by, 36
music by, 6, 24, 31, 32, 36, 41, 65, 518
novels by, 20, 67, 68, 70–73, 84–86, 93, 103–109, 246, 476, 492, 518, 535
painting by, 6, 9, 10, 16, 24, 25, 27, 32, 41
pay of, 49, 54, 63, 75, 89, 101, 245, 368, 369, 371, 390, 414, 439, 459, 460, 466, 499, 512, 513
pensions of, 213, 245, 285, 318, 356, 369, 370, 512, 513
phonetic spelling of (musaic spelling), 53, 508
poetry of, 6, 27, 28, 30, 36, 37, 39, 44–48, 50–53, 71, 76–78, 88, 90, 91, 95–99, 103–105, 185–187, 213, 217, 218, 220, 225, 226, 290, 493, 506, 507, 517, 519, 535
ponies of, 58, 65, 68, 73, 87, 91, 101
practice of (medical), 56–59, 66, 91, 369, 466, 512
prizes won by, 109, 129, 469, 476–483, 525
researches of (medical). See Malaria, Mosquitoes, Pathometry, and, 526–534
service of, active, 86–89, 518–522
service of, Indian, 40, 89, 179, 199, 200, 212, 245, 255, 285, 356, 369
service of, at Liverpool, 369, 459, 466, 511–513
service of, at War Office and Pensions' Ministry, 518, 519
torpedoing experience of, 519–522
writings of, 526–536
writings of, reviewed, 48, 85, 86, 106–109, 505–507, 518
Ross, Mr. R. C., 111, 515
Ross, Miss S., 93, 518
Ross, W., 3, 4
Ross, Col. W. A., 5, 6, 7, 8, 32, 415, 491
Rowe, Capt. and Mrs., 102
Royal Institution, 395
Royal Society, The, 283, 284, 310, 315–317, 321, 336, 337, 332, 374, 376, 418, 419, 421–423, 429, 440, 484–487, 511
Royal Southern Hospital, 368, 375
Ruffer, Dr. M. A., 327, 328
Russell, Sir E. (Lord), 505, 506
Russia, 509–511
Rutherford, Mr. W. W., 482
Ryde, 15, 21, 22, 24, 25, 32, 82

St. Bartholomew's Hospital, 27, 29, 30, 33, 127, 376
St. Helena, 21
St. Louis Congress, 8, 491, 492
Sakharoff, N., 131, 194, 337, 400, 480
Salonika, 416
Salt Solution, 140
Sambon, Dr. L. W., 119, 401, 417, 419, 485
Sampson, Dr. J., 505
Savas, Prof. C., 494–497, 514
Savory, Sir B., 33
Scavengers, 186, 187
Schafer, Prof. Sir E. A. S., 469
Schaudinn, F., 313, 377
Scheer, Dr. A. van der, 481
Schilling, Prof. C., 422, 482
Schools of Tropical Medicine, 354, 366, 367, 436
Liverpool, 367–371, 374, 392, 434, 474, 502
London, 371, 376, 434, 515
Schüffner, W., 417
Science Progress, 502, 508, 515, 516
Scotland, 83, 455, 456, 465, 469, 488, 489, 511
Scott, Mr. D., 48, 506
Secunderabad, 101, 134, 187, 199, 214–240, 381
Seely, Rt. Hon. Col. J. E. B., 508, 511, 513
Setting Sun, The, 508, 509, 535
Seychelles, 499
Shaw, Mr., 463
Sherrington, Prof. (Sir) C. S., 368, 376
Shipley, Dr. (Sir) A. E., 395, 497
Shrewsbury, 34, 35
Sibthorpe, Surg.-Gen., 78, 201, 203, 212, 243
Sierra Leone, 123, 333, 352, 374, 378–389, 394, 408, 414, 415, 423, 429, 433, 441–444, 449–453, 461–466, 485–488
Sigur Ghat, 203–211, 216, 218, 220, 221, 222, 228, 237, 258, 278, 402, 405
Simond, R. D., 197, 251, 321
Simpson, Dr. G. C. E., 511
Simpson, Prof. W. J., 306, 487
Sleeman, H., 7, 32

Sleeman, Col. J. L., 7
Sleeman, Sir W., 7
Slight, Mr. A. M., 180, 182
Smales, Maj. W. C., 519
Smith, Sir T., 33
Smith, Dr. Theobald, 123, 193
Smith, Prof. Sir W. R., 83, 475
Smith, Wolsey, Mr. and Mrs., 95
Smyth, Dr. J., 232, 233, 247, 253, 288, 329
Society of Arts, 430
Space, Matter, and Motion, 54–56
Spain, 513
Spicer, Sir A., 509
Spirit of Storm, The, 67, 103, 105–109, 492, 535
Springhill School, 23, 26, 28
Stack, Mr. W., 517
Staff Surgeoncy, Bangalore, 91, 100, 101, 191
Steele, Mr. B., 494–497, 514
Steeves, Dr. G. W., 470, 515
Stegomyia (or *Ædes*, a genus of Mosquitoes), 58, 89, 136–151, 156, 256, 257, 471, 472
calopus (or *fasciata*), 124, 219, 424–426
scutellaris, 169–177, 187, 209, 210, 219–239, 256, 257
gregarines, parasites of, 169–178, 187
Stephens, Dr. J. W. W., 322, 382, 422, 423, 463, 481
Sternberg, Surg.-Gen., 330, 331, 332, 424, 427
Stockholm, 456, 476–483, 507
Stott, Mr. W., 489
Strachan, Dr. H. W. W., 378, 386, 388, 433, 444–448
Strathcona, Lord, 499
Strong, Prof., 506
Sturmer, Dr., 188, 191, 216
Suez, Comp. du Canal Maritime de, 470–474
Sundberg, Prof. K., 478
Surbiton, 105
Surgeons, R. Coll. of, 37, 38, 82, 440
Surveyor, Dr., 191
Swarm-spores, see Flagellulæ and *Crithidia*.
Swanzy, Mr. F., 435, 439, 457, 458
Swekendain, 88
Switzerland, 81, 82, 103–105

Tabanus (horse-flies), 254, 280
Tait, Mr., 74, 93–95
Tait, Prof. P. G., 58, 94, 95, 415
Tambaki, Hon. C., 448
Taranto, 519
Taylor, Dr. L., 438–444, 457, 458, 460–466, 481, 485–487
Teneriffe, 440, 441
Terrigi, 119, 397
Territorial Force, 518
Terzi, Sgr., 417
Texas Cattle Fever, 123, 401, 404–406
Thayer, Dr., 480, 491
Theobald, Mr. F. V., 481, 484
Theory of Happenings, 505
Thick-Film Method, 168, 189, 475
Thin, Dr., 248, 253, 254, 257, 261, 277, 288, 296, 330, 332, 411
Thomas, Capt., 102
Thomas, Mr. G., 40, 64
Thomas, Mr. H. S., 74
Thomas, Mrs. M. A., 16, 64, 376
Tommasi-Crudeli, C., 119
Thomson, Dr. D., 511
Thomson, Dr. J. C., 433, 455
Thomson, Dr. J. G., 511
Thornhill, Dr. H., 262, 329
Thought reading, 69
Times, The, 392–394, 456, 487
Todd, Dr. J. L., 493
Torti, 118
Trichocephalus, intestinal parasites, 281
Tropical Medicine, Bureau of, 511
Tropical Medicine, Annals of, 502
Tropical Sanitation, Professorship of, 512
Tropical Sanitation, Fund, 439, 457, 460, 485–487
Trypanosomes, 221, 222, 246, 255, 294, 457
Tsar, The, and Tsaritsa, 509, 510
Tugwell, Bishop, 448
Tuson, Capt., 68
Tytler, Mrs. A. A., 5
Tytler, Gen. J. A., 5, 8, 9, 11, 12

United Planters' Association, 201
University of Liverpool, 372, 378

Vacuum-theorists, 430, 489, 501, 504
Van't Hoff, Prof. J. H., 477
Varro, 118
Venezelos, M., 514
Verb-functions. See Operative Algebra
Vermicules (moving zygotes), 267, 268, 271, 281, 313. See *Plasmodia*
Vesuvius, 80, 81
Vivisector Vivisected, The, 49
Vizianagram, 46–49, 53

Waddell, Surg.-Maj., 265
Wagner, R., 32, 36
Waldorf Astor Hotel, 492
Wales, 470, 511
Wallace, Sir D. M., 509
Warner, Sir L., 179–181
War Office, 519
Warren, Mr., 417

Warren, Sir H., 506, 517
Waterspouts, 71
Watson, Rev. J., 484
Watson, Dr. M., 432, 440, 490
Weardale, Lord, 509
Weber, Dr. P., 519
Webb, Sir A. L. A., 519
Weeks, Mr. H. C., 492
West Africa, 357, 485
White, Mr. Arnold, 522
White, Dr. Carr, 191
Whitehead, Prof. A. N., 415
Williams, Dr. (Sir) Dawson, 253, 377
Williams, Mrs. S., 446
Williamson, Dr. G., 439
Wilmot, H., 21, 22, 25
Wilmot, W. B., 21, 23–25
Woodhead, Prof. Sir S., 395
Woodwark, Dr. A. S., 519
Worthington, Sir E. S., 519
Wright, Dr. (Sir) A. E., 327

Yate, Col. Sir C. E., 509
Yates, Miss E., 294, 515
Yellow Fever, 123, 124, 424–426
Young, Dr. J. M., 455
Young, Sir Mackworth, 181

Zeeman, Prof. P., 477, 479, 480
Zeri, A., 122
Ziemann, Dr., 480, 481
Zygotes (fertilised malaria parasites, pigmented cells). See *Plasmodia*

26

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